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THE NEW PHYSICS AND CHEMISTRY

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THE NEW PHYSICS AND CHEMISTRY

A SERIES OF POPULAR ESSAYS ON PHYSICAL AND CHEMICAL SUBJECTS

BY

W. A. SHENSTONE, F.R.S.

LONDON
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1906

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PREFACE

These Essays were published originally in the *Cornhill Magazine*, and are reprinted at the suggestion of the Editor and by the kind consent of Messrs. Smith, Elder & Co. They were written in no particular order, at irregular intervals, and each Essay was intended to be complete in itself. The result is that a few repetitions will be found here and there. I have not attempted to remove this feature, partly because these repetitions are not numerous, partly because it seems likely that the Essays will be read one by one, like Mr. Anstey's "Salted Almonds," in which case they may be found useful.

Some one has said that an Essayist is, or ought to be, an Ambassador from the realms of Literature, Science or Art. I take it to be the business of an Ambassador from these regions to enlighten rather than to teach; that he should treat his subjects broadly rather than minutely, avoid rather than revel in details, and put aside all technicalities if he can create the impression he desires without them. He may, I think, even invent illustrations and use his inventions in the place of real cases, when necessary, in order to keep clear of the complexities which so often overlie scientific investigations. I have, I must confess, exercised my functions as an Ambassador in a liberal spirit; hence these essays are pictures rather than photographs. Nevertheless, these pictures are not dream-pictures, but will give, I believe, true ideas of the great researches and hypotheses with which they deal.

Nothing is more characteristic of modern Physics and Chemistry, than the audacity and success with which theory is employed by the great masters, and these essays give some account of several of the most startling examples of modern theory. It may be helpful, therefore, if I remind my readers that we must not confuse these tools of Science with the work done by their aid, but should look upon them as searchlights which cast light into dark places and enable us to see, sometimes plainly, sometimes only in dim outline, much that would remain hidden if we were denied their aid. But we must not fall into a frequent error and adopt these theories as articles of belief. We must never forget that they are only tools, and, like all tools, liable to be superseded at short notice by better and newer ones.

We are often told that we live in a material age, that the days of chivalry are gone, that even Science devotes herself to-day to the merely useful, and is apt to neglect the search after abstract truth. Perhaps these imperfect recitals of the progress of some great researches, including, as they do, many splendid contributions made in recent years, may serve as a reminder that though Science reveals herself to most of us chiefly through her more obviously useful discoveries and inventions, those who look for them will still find among us not a few men and women as ready as any of their predecessors to devote days and nights to hard labour for no other fee than the hope of discovering a new truth, overthrowing an ancient error, or extending in some other way the boundaries of knowledge.

W. A. SHENSTONE.

CLIFTON COLLEGE,
October 1906.

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THE NEW PHYSICS AND CHEMISTRY

MATTER, MOTION, AND MOLECULES

> "Change is everywhere; everything is and is not. There is no stability. Even in the same river one cannot bathe twice, nor even once."

IN his Presidential Address delivered before the members of the British Association at their meeting in South Africa, Professor Darwin recently reminded us of the tremendous scope of modern speculations as to the constitution of matter. At one end of the scale we contemplate in the heavens arrangements of matter on schemes so vast that no mind can picture them. At the other end, according to the "corpuscular hypothesis," we find in the chemical atoms yet other constellations, which, though they are individually smaller than the heavenly arrangements, are not less complex nor less numerous; whilst in between these two extremes matter occurs in a multitude of phases, physical and chemical, so vast and so varied that the task of discovering their relations has taxed to the utmost the powers of the greatest minds right through the ages. It is to this middle field that the "molecular kinetic theory," or, briefly, the "kinetic theory," more particularly applies, helping us first to get a picture of the relations of the solid, liquid, and gaseous states of matter, and then, as will be shown in some of the following Essays, to unravel some of the complexities which underlie chemical phenomena.

Any one who has watched a thermometer plunged in a vessel filled with crushed ice will remember how steadily the mercury stands at the zero point until all or nearly all the ice has melted. He may remember, too, that this is equally the case whether the ice melts slowly or quickly; that it makes no difference whether the experiment be made in cool air in the open, or in a warm room, and that even if the vessel be placed over a flame the result is the same; that in any case and in every case, provided only that the experimenter does not work with undue haste, and that a sufficient supply of ice is available, it is impossible under ordinary circumstances to raise the temperature of ice or of a mixture of ice and water above 0° C. Now, this curious fact, which was discovered by Dr. Hooke in the latter part of the seventeenth century, does not stand alone. Other solids in thousands behave in a similar manner, and the change of a liquid into a steam during the process of boiling is accompanied by corresponding phenomena. Boil water, for example, how you may, quickly or slowly, by means of a coal fire, a gas flame, or an electric furnace, in every case you will find it impossible to raise its temperature above a fixed point. This point may vary slightly from day to day, rising a little when the barometer is high, falling a little when the barometer is low, but, at or near the sea-level, it is never very much above nor very much below 100° on the Centigrade scale.

These facts about the melting of ice and the boiling of water, discovered, as I have said, by Hooke in the seventeenth century, were used by Newton as the standards by which the "fixed points" of thermometers might most conveniently be determined ; but their meaning remained hidden till it was revealed by Dr. Black in a memoir read before a literary society in Glasgow in 1760. This memoir, a classic among the classics, forms one of the foundation-stones of the "kinetic theory"; that is to say, of modern physics. Put briefly, Dr. Black's explanation was as follows: The heat which disappears when ice is melted combines with the ice, just as one element combines with another when a compound is formed. Water is a sort of compound of ice and *caloric*, in which these two are united in definite doses like the elements in a chemical compound.

We no longer regard heat, or *caloric*, as only a more subtle, imponderable form of matter. Hence we no longer consider water to be a *compound* of ice and caloric, in the sense in which we believe ice to be a compound of hydrogen and oxygen. And thus Black's hypothesis takes to-day a different form. But we agree with Black that in the melting of ice, or any other solid, a definite dose of something—call it heat—actually is received by the solid and in some way hidden, or rendered latent, within it. Thus Black's idea still lives, and Black himself must be regarded as the earliest of the founders of the great theory which forms the subject of this article. His name must be associated with those of John Dalton of Manchester and Amadeo Avogadro of Turin, who contributed the atomic-molecular hypothesis, and with those of the great physicists of the Victorian era who developed the modern doctrine of "energy."

I must now ask those of my readers who are not already equipped with clear ideas as to the meaning of the terms "matter" and "energy" to give their attention for a few moments to the two fundamental conceptions which correspond to these terms. Unless we are clear about these, much that follows will hardly be intelligible, and we shall fail to grasp even the broader features of the mind-pictures by which physicists seek to indicate the unseen causes of the visible differences which distinguish the three typical forms of matter. We need not, however, dig very deep in this part of the field. It is not necessary for our present purpose to concern ourselves with the refined speculations which go behind the atoms of the chemist, and seek to identify matter with electricity or to trace its origin back to the "inevitable ether." For the moment, at least, we need only remember that, practically speaking, matter is indestructible; that every particle of it, whether great or small, simple or compound, exhibits a kind of passivity or dogged perseverance, known as *inertia,* in virtue of which, as Newton taught, every body "perseveres in its state of rest or of uniform motion" unless it is compelled by some force to change that state ; that its properties suggest it exists in particles, called molecules, so small that about 2,000,000,000,000,000,000,000 of them may exist in a single drop of water ; and, finally, that these molecules, small as they are, are not indivisible, even in the hands of the chemist, but are built up of still smaller and chemically indivisible parts called atoms. Further than this we need not go. Whether these smallest particles, like the atoms of Newton, are solid, massy, hard, impenetrable portions of matter, whether they are "mere points without extension but surrounded by spheres of attractive and repulsive force," like the atoms of Boscovich, whether they are particles carrying electric charges or particles of electricity itself, and whether they are really indivisible, as chemists have long assumed, does not concern us now. Some of these matters will be discussed in subsequent essays, and all may safely be neglected here.

Turning our attention to energy and its laws, we must ask ourselves the question, "What is energy ?" Unfortunately this question, simple as it seems, is no easier to answer than the equally simple and equally important question, "What is matter?" And I fear that if one of my readers should spend a few weeks studying what has been written on the subject, he would rise from his inquiries still asking himself, "What is energy?" Nevertheless, his time would not have been wasted. As the result of his labours he would be convinced that energy, whatever it may be, does exist; that though it differs from matter most strikingly in its more obvious manifestations, it is, like matter, measurable, also equally indestructible, and, if I may use the term in this sense, equally polymorphous, though polymorphous with a difference, presenting itself alternately as "kinetic energy or energy of motion," "strain energy," "radiant energy," "heat energy," "electric energy," "chemical energy"—if the latter really be distinct from electric energy, which seems doubtful—and almost eluding us at times by entering on a hidden or latent phase, as when heat energy disappears in the melting of a solid or the boiling of a liquid. In this last state the physicist labels it provisionally "potential energy," or "energy of position," pending the time when he shall discover what really becomes of it in this phase, and whether it is not then, as he suspects, stored away in the ether, still probably in the kinetic form. One other fact about energy would also have become clear, viz. that "matter" is not, as at first he might have supposed, the only or even the chief vehicle of energy; that, on the contrary, vast quantities of energy pass perpetually in every direction through the boundless ocean of ether which fills all space and permeates every one of the sparsely scattered specks of matter which bulk so large in our philosophy and make up the sensible part of the material universe.

The conviction that some single cause underlies the seemingly diverse phenomena of mechanics, heat, light, &c., which now plays so important a rôle in science, has only assumed its present position during comparatively recent years. Even so late as the early part of the nineteenth century the possibility that this might be the case was but vaguely foreseen, and it was not until the middle of that century that ideas on the subject really began to crystallise. But by 1845 a start had been made, and we then find Faraday saying, "I have long held an opinion, almost amounting to conviction, in common, I believe, with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and materially dependent that they are convertible, as it were, one into another, and possess equivalents of power in their action." Nor was this a mere pious opinion. When Faraday wrote these words not only had evidence of the convertibility of the forces of nature been accumulated, and a beginning been made in the work of determining their quantitative relations; but Joule, of Manchester, a sometime pupil of John Dalton's, already had commenced the classical research by which he showed us how we may determine what has been called the "mechanical equivalent of heat"—that is, the "rate of exchange" between kinetic energy and heat energy when either of these replaces the other. In this research Joule ascertained that the kinetic energy due to the fall of 772 lb. of matter through 1 foot, when transformed into heat, will raise the temperature of one pound of water through one degree Fahrenheit, for example, from 55° F. to 56° F. or from 56° F. to 57° F., and so on; the exact rate of exchange depending a little on the temperature of the water at the beginning of the experiment and, since the force of gravity varies in different localities, on the place at which the experiment is made. This result has been confirmed repeatedly, and is now generally admitted to constitute a sufficient demonstration of the validity of the principle of the conservation of energy.

Of all the forms of energy enumerated above, only two need be considered in connection with our present purpose, of reconstructing the theory of matter which pictures the hidden mechanics of the transformations in which solids are converted into liquids and liquids into gases or vapours. These two are kinetic energy and potential energy; but about these we must be very clear.

Every body that is in motion possesses kinetic energy, and can do work in virtue of that energy; the amount of this work depending, first, on the mass of the body, and, secondly, on the velocity with which it moves in such a way that if we multiply half the mass of the body in pounds by the square of its velocity in feet per second[1] we shall have a measure of its "kinetic energy," and be able to compare this quantity with the kinetic energy of any other moving body, provided that we know the mass of the latter and the velocity with which it moves. This statement applies equally to large masses and to small—to the sun, to the earth, and to the minute particles or molecules of which every solid, every liquid, and every gas is built up. When the work which corresponds to the product of half the mass and the square of the velocity of a moving body has been done in some way, as by lifting a weight, heating water, or turning a wheel, its kinetic energy is exhausted and the body itself ceases to move. It then seems to be devoid of energy, just so much dead, or, let us say, bankrupt matter, which can only be revivified if man should intervene, or if some convulsion of nature should start it on a new career. Now the question is, Does this view of the state of a body which once was in motion but has come to rest truly correspond to the facts of the case?

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[1] The *foot-pound*, the unit chiefly employed in this country, gives the work done—that is, the energy expended—in lifting a one-pound weight one foot in opposition to gravity. Other units can, of course, be made use of.

Well, in a very restricted sense perhaps it may do so, for it may be that the store of kinetic energy with which the body was endowed when it was set in motion has now departed from it, taken some new form, and started on some new career, perhaps as heat, perhaps as light; perhaps, again, as motion, as, for example, when one billiard ball striking another stops dead, while the second travels onward. But it may be, on the other hand, that the original stock of kinetic energy has merely been transformed into an equivalent amount of "energy of position," or potential energy. The following illustration will make this clear. Suppose that a stone is thrown vertically from the earth with an initial velocity of 9.8 mètres per second. Then as it rises, doing work against gravity, its kinetic energy will gradually diminish, and it will move upwards more slowly than at first, and then yet more slowly, until it comes to rest at a point 4.9 mètres above that from which it started. If the velocity of the stone at starting had been twice as great, viz. 19.6 mètres per second, then it would have risen not twice, but four times as high as before, viz. 19.6 mètres; and so in proportion in other cases. But, however great the initial velocity may have been, the end will be the same: sooner or later a point will be reached at which all motion will cease. At this point, when the stone remains poised for an instant, it has exhausted the kinetic energy with which it started.

Are we to conclude that at the top of its flight the stone is a physical bankrupt, that the stock of energy with which it started is gone once and for ever out of existence? By no means. Although every trace of kinetic energy has disappeared from the stone, so that for the moment it hangs, like Mahomet's coffin, poised between earth and heaven, and can do no further work, its total stock of energy is not a jot or tittle smaller than at first; it has gained, as we shall see in a moment, an amount of potential energy exactly equal to the kinetic energy which has just disappeared. For if at the moment of its highest flight a skilful experimenter should support the stone for an instant, join to it by a weightless string running over a frictionless pulley a second stone of equal weight, and then allow it to start on a downward career, we should find that the first stone in falling would be able to raise a mass equal to itself through a distance equal to its own fall; that is, to a height equal to that to which it was itself raised by the kinetic energy with which it started. In short, at the moment when its kinetic energy was exhausted the stone must have possessed an amount of energy of another order, viz. potential energy, exactly corresponding to the quantity of kinetic energy which disappeared in the previous operation.

I need hardly say that the exact experiment described in the last paragraph could not be carried out. We could not stop the moving body exactly at the right moment. We could not get two stones of precisely equal masses. And if these difficulties were overcome, there would be some friction between the pulley and the string; whilst other complications would arise owing to the fact that we could not obtain a weightless string to connect the two stones used in our experiment. Hence the physicist cannot demonstrate absolutely the truth of the "principle of the conservation of energy" in this way, or by any other possible experiment, any more than the chemist can demonstrate the truth of the statement that no matter is ever created or destroyed in a chemical change. But if we make due allowance for inevitable errors of experiment, the results of many careful investigations lead us to conclude that energy exists in many forms, and that one form of energy may be converted into another form and the process reversed without loss. In short, on the evidence as it stands to-day, we may assume that there is in the universe a certain quantity of energy, and that it is beyond our power to add to or diminish that quantity. We may be wrong when we assert that energy and matter are indestructible and uncreatable; this must be admitted, if only to avoid dogma, but on the evidence before us we are bound to assume that neither matter nor energy can be created, and that neither matter nor energy can be destroyed by any means at our disposal at this moment.

We find, then, that a body which has spent its kinetic energy in doing work is not of necessity devoid of energy. Indeed, if we look into the matter closely, we find that, as far as we can see, no body ever is really without large stores of energy of one kind or another. Consider, for example, such a case as that of a stone or a piece of coal which has fallen and lies motionless upon the ground. Is this bankrupt of energy? Clearly it is not so. In the first place, the stone or coal may lie upon a hillside, and in that case obviously it possesses energy of position, which would become kinetic if we set it rolling down the hill. Secondly, the stone or coal certainly would not be absolutely cold, and therefore would possess some store of heat energy, which might be made useful by means of suitable arrangements; whilst sufficient energy is locked up in every pound of coal to raise the temperature of nearly a hundred pounds of water from the freezing-point to the boiling-point, and this could easily be made available by burning the coal in a fire. Then, again, to extend the range of our ideas for a moment, a stone or piece of coal, however still and immovable it may seem to us, is not still, but is rushing through space, in company with everything around it, with an enormous velocity; and, finally, if our present views on the nature of matter are correct, the molecules of which every object is built up are composed of atoms, every one of these atoms is a congeries of corpuscles, or electrons, and these atoms and corpuscles are in a state of motion. Is it not clear, then, that in experiments like those described above we deal with only a pigmy portion of the total energy of the arrangements of matter studied? Is it not clear that vast stores of energy must exist, hidden from our eyes, not only in the universe, but even in fragments of stone so small that we may toss them unheedingly from hand to hand, or in a single cup of the coldest water?

According to the kinetic theory of matter, we must consider that every solid body, every liquid, and every bubble of every gas consists of a multitude of separate particles, or molecules. In the case of solids and liquids we must suppose, further, that these molecules are held together by some sort of attraction; for in order to expand a solid or a liquid—that is, to spread apart its molecules—we must do work upon it. This work may be done by supplying it with heat. Part of this heat must be converted into kinetic energy of the molecules, because when a body expands its molecules must fly asunder. Probably another part is transformed into potential energy, because work must be done in order to overcome the attraction between the molecules, just as it is when a stone is thrown vertically upwards from the earth.

We know that every hot body radiates energy. If the body be very hot the radiations may be such as can affect our eyes, when we say it is red hot or white hot, as the case may be; whilst at lower temperatures we can detect the radiations in other ways, for example, by means of thermometers. These radiations are due to waves, which implies vibratory motion of some kind in the heated body from which they come. The vibrations of the heated body may be vibrations of its molecules as individuals or vibrations of the constituent atoms of its molecules, and either of these might vibrate in various ways, which we need not consider here. But whatever the source of the vibrations may be, we must regard radiant energy, in every case, as a mixture of vibrations. To put the point a little differently, heat may be traced back to the kinetic and potential energies of vibrating particles of matter. If in any case we increase these energies, we raise the temperature of the body. If we allow them to diminish, as, for example, by radiation, its temperature will fall.

For our present purpose it is not necessary to consider the nature of the atoms or their arrangement in molecules, except so far as these relate to the three states of matter. The fact that many solids, such as jewels and flint instruments, retain their shapes practically unaltered for hundreds, and even thousands of years, leads us to conclude that the molecules in an ideal solid only move under great restraint. In the case of very hard solids, like diamond, flint, and ruby, it might be supposed, for example, that each molecule oscillates about a mean position, but the molecules cannot be supposed to make long excursions from their original positions in the body. The same remark applies to substances like gold and lead, which are not so hard as diamond, but not to an equal extent, for we know that when these two metals are brought into close contact their particles mingle perceptibly, though only very slowly, which shows that some of their molecules, at any rate, possess a certain amount of freedom.

If you place a layer of a solution of a coloured salt, such as blue vitriol, at the bottom of a deep vase, then gently pour water above it, so as to keep the two liquids quite distinct, place the vessel where it will be undisturbed by tremors, and watch what happens, you will become aware in a few days or hours that the two liquids are mixing spontaneously, and sooner or later you will find that your vase contains a uniform mixture of them. The process is slow — far slower, for example, than that by which the scent of a flower spreads through still air; nevertheless, the readiness with which many liquids mix when thus placed in contact compels us to conclude that the molecules in these are under far less restraint than those of solids. It seems clear, in short, that the molecules in a liquid can slide over one another and move about freely, if slowly, in all parts of the liquid. The slow rates at which liquids mingle may be attributed to the closeness with which the molecules are packed; for close packing would cause molecules to jostle one another, like people in a panic-stricken crowd in the street who have ceased to observe the rules of the road. We may consider, therefore, that liquids, like solids, consist of molecules in motion, but that whilst the molecules of a solid, except, perhaps, some of those which are near its free surface, can only oscillate about mean positions, even when the solid is very hot, those of a liquid are not thus tied, but are free to move from point to point within the liquid. Even in the case of liquids, however, the molecules must act very considerably on one another. The obvious fact that molecules in liquids need no external restraint to hold them together, and the circumstance that some liquids are scarcely more volatile than solids, so that the evaporation of liquids, which must be ascribed to the escape of molecules from their surfaces, often proceeds very slowly, both combine to teach us this.

The case of a gas differs strikingly from that of a solid or a liquid. In the first place, liquids when they turn into gases expand vastly, a single cubic inch of water yielding, for example, about sixteen hundred cubic inches of steam. Hence the molecules in a gas are so widely separated that, as has been established by Dr. Joule and others, cohesion is almost absent. We may think of a bubble of a gas as consisting of a swarm of extremely minute particles flying about incessantly, in every direction, in straight lines, with immense velocities, except when they come into collision with one another. Think of the particles which you see playing in the air when a beam of light passes across a darkened room, but substitute for the relatively large dust particles molecules so small that about sixty million, million, million of them will go inside a lady's thimble ; think of these ultra-microscopic particles as moving not sluggishly, like dust in the air, but at the rate of seventy miles a minute, so that every molecule comes into collision with some other molecule about eighteen thousand million times in every second of its existence, and you will have a fairly satisfactory mind-picture of the gas hydrogen.

Here, then, we have pictures which may be taken to represent the constitution of matter in the solid, liquid, and gaseous forms drawn by the light shed by the "kinetic theory," and it only remains to return for a moment to Dr. Black's discoveries, with the hope that by studying these under the illumination afforded by the same powerful searchlight we may gain a better idea than he could give us of the nature of latent heat, and of the processes by which a solid is transformed by heat into a liquid and a liquid into a gas. Picture to yourself a piece of ice. Think of its molecules as held together by some cohesive force which allows each to move to a limited extent, to rotate, perhaps, on an axis like a top when it hums ; or to move about a fixed point, to oscillate, for example, backwards and forwards like the pendulum of a clock, but which prevents them from moving from one part of the solid to another, keeps every molecule from straying far from any position in which it may find itself. Now, suppose you bring this piece of ice near some source of heat, such as a fire, so that energy may reach it in the form of radiant heat.

What will happen? Well, if the ice is cold, if its temperature is a few degrees below the freezing-point, for example, we can imagine that the first supplies of energy which reach it will be used to make its molecules move more quickly than before. They might rotate more quickly or oscillate more rapidly. But as soon as the temperature of the ice was raised in this way to the melting-point a change would set in: some of the ice would melt, then more and more, till all was melted; and all the while, unless we hurry the process quite unreasonably, the temperature of the ice and the water would remain stationary till all the ice was melted. Now, what becomes of the heat energy which thus disappears? According to Dr. Black, heat is a subtle substance and combines with the ice. According to the kinetic theory, the heat energy does work, overcoming the forces which in solids hold the molecules together, and enabling the latter to move as they do in liquids amongst one another. When all the ice is melted a new state of affairs sets in. If at this stage we continue to supply heat to the water, we again hurry up the motions of its molecules and thereby raise its temperature, increasing its power of radiating energy as we go on, until another check occurs at the boiling-point, when heat energy again disappears in great quantities whilst the water boils. At this final stage, as you will see if you consider what has been said about gases, work is again done and energy spent in overcoming the remaining attractions between the molecules, which are set free and started untrammelled on new careers in the gaseous state.

You will now understand the nature of the so-called "latent heat." The molecules of a solid, and in a less degree those of a liquid, are held together by attractive forces. In order to overcome these forces work must be done, just as it must be done when any two bodies, such as a stone and the earth, are drawn apart. Now we have seen that the kinetic energy which disappears when a weight is lifted is replaced by an equal amount of potential energy. Here, then, is the explanation of the latent heat of fusion of solids and the latent heat of evaporation of liquids. The heat energy which disappears when we melt ice or boil water does work, overcomes the attractions between the molecules of the solid or liquid, starts these molecules on new careers and becomes potential. According to this view, it should be possible to recover this energy undiminished, or nearly undiminished, in quantity by reversing the processes of evaporation and liquefaction. And so it is. It has been done times without number.

SOME RECENT SPECULATIONS ON THE CONSTITUTION OF MATTER

In the days of Ptolemy Euergetes, about two hundred years before the beginning of the Christian era, a great geographer, Eratosthenes of Cyrene, busied himself with an attempt to measure the biggest thing he knew, viz. the Earth. Centuries have piled themselves on centuries since then, all but a few fragments of the works of Eratosthenes have perished, and still "Science is Measurement." Only, to-day our measurements cover a wider field, and besides weighing and measuring the earth, we seek to discover the dimensions of the minute "atoms" of which worlds are built up. Nor does modern Science stop even here. For after revolutionising our ideas about the relations of the atoms in chemical compounds, Physicists have gone on to deny the indivisibility of these atoms, and to-day they ask us to believe that particles far smaller than hydrogen atoms exist, which also must be measured.

Are the atoms of the chemical elements indivisible? Are they the ultimate, the eternal particles of matter? This is the question which the physicist has sprung upon us.

I am going to try to tell, in non-technical language, something about the astonishing researches and speculations that have brought us again face to face with this fundamental question, and compelled us to re-open discussions which some have been disposed to regard as definitely closed, at any rate for the present. Those who would dig deeper than I may go into these big matters will find help in a discourse on "The Existence of Bodies smaller than Atoms" delivered before the Royal Institution by Professor J. J. Thomson on April 19, 1901, in Sir Oliver Lodge's address on "Electrons," which may be found in the proceedings of the Institution of Electrical Engineers for 1903, and in Professor Rutherford's contributions to recent numbers of the *Philosophical Magazine*.

When Eratosthenes set out to measure the dimensions of the earth he adopted the doctrine, which, by that time, was pretty generally accepted by astronomers, that the world is a globe. And the value of the results deduced from his measurements depends upon the truth of that doctrine. Similarly, the Chemist of to-day assumes the truth of the theory which tells us that all matter is made up of extremely minute, indivisible particles, called atoms, and that these atoms combine together in various numbers to form larger particles, called molecules, of the multitude of elements and compounds with which he is acquainted. If this hypothesis be wrong, if the atoms of the chemist be not indivisible, then all that depends upon it falls to the ground. Hence the importance of the question raised by the most recent revelations of Physical Science.

It would complicate matters needlessly were I to attempt to tell how the molecular-atomic hypothesis has come to occupy its present position. I must, therefore, ask my readers to accept the statement that chemistry has taught that each of the eighty chemical elements exists in minute atoms; and that until a little while ago we had no reason to believe that any one of these atoms had ever been subdivided or broken in any laboratory experiment, or indeed in any terrestrial event that we are acquainted with; that so far as we could tell, and for all practical purposes, atoms were uncreatable, indestructible, to be brief, eternal.

For more than half a century the majority of chemists and physicists have accepted this conception of the constitution of matter as a working hypothesis which was not contradicted by any well established fact. And so strong has been the position of these atoms and molecules that Loschmidt, Johnstone Stoney, and Lord Kelvin have taken the trouble to calculate their approximate sizes and masses.[1] Their results show that in the case of gases, 20,000,000,000,000,000,000 molecules only occupy one cubic centimetre, at 0° C. and the average barometric pressure. When I say that an average thimble will hold three cubic centimetres, and that in the case of hydrogen these 20,000,000,000,000,000,000 molecules, each containing two atoms, only weigh about the one ten-thousandth part of a gram, that is about the one seven-hundredth part of a grain, some idea will be gained of their extreme minuteness. And yet as the result of the work of the last quarter of a century physicists have come to the conclusion that in the Crookes' vacuum tube, and elsewhere, we have to reckon with far smaller bodies than these almost inconceivably minute chemical atoms. These latter bodies are called "electrons."

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[1] These were calculated by methods which involved the making of certain assumptions.

The term Electron was originally applied to a particular quantity of electricity. When an electric current is passed into an aqueous solution of a mineral acid by means of two metallic plates or electrodes, the constituents of the acid part company and move to the two electrodes; atoms of hydrogen going to one electrode, the cathode, and the other constituents of the acid, according to its nature, to the other electrode, which is called the anode. But further, each atom of hydrogen which goes to the cathode carries with it a positive charge of electricity, and similarly the other constituents of the acid carry to the anode an equal amount of electricity of the opposite kind.

The quantity of electricity carried to the cathode by each gram of hydrogen is found to be the same in every experiment, no matter what acid we may decompose. And from what has been said before, it will be seen that, subject to the truth of certain assumptions, the approximate mass of an atom of hydrogen is also known.

Hence the approximate charge carried by each atom of hydrogen can be calculated.

Now if it be true that electricity requires matter to carry it, and that hydrogen atoms are the smallest particles of matter and indivisible, it follows that we cannot have a smaller quantity of electricity than that carried by an atom of hydrogen. That is to say, the quantity of electricity carried by an atom of hydrogen cannot be divided.

This indivisible quantity of electricity has been called "an atom of electricity." And Dr. Johnstone Stoney proposed for it the name "electron."

As we shall learn from a subsequent essay, charged atoms are called *ions.* So that chemists and physicists now have to deal with atoms, the indivisible particles of the elements; electrons, the indivisible quantities of electricity; and ions, which are atoms, or groups of atoms, carrying one or more electrons. We must remember that every atom of hydrogen carries in electrolysis a particular quantity, or charge, of electricity, and that every other atom, or group of atoms, carries either this same quantity of electricity or a multiple of it, but never less; that the quantity of electricity carried by an atom of hydrogen behaves as if it were as indivisible as the atoms themselves.

There is one other point. The atoms and their charges are not inseparable. Ions part with their charges in electrolysis, and also under other circumstances.

We must now turn our attention to the phenomena of electric discharge in gases. If we obtain a glass tube ten or twelve inches in length, seal into its ends two platinum wires, connect these wires to the anode and cathode of an electrical machine, start the machine and then gradually remove the air from our tube by means of a first-rate air-pump, we may observe the following phenomena in the following order:—

Before we begin to exhaust the tube the electricity refuses to pass between the wires, because it is unable to overcome the resistance of the air. But presently, as the air is removed, sparks begin to pass which spread out as it were, till by-and-by the tube is filled with a beautiful glow. At still higher exhaustions the uniform glow breaks up into a succession of transverse luminous discs, or striations, and at one of the wires—the cathode —a dark space shows itself in front of the cathode. This dark space grows as we continue to exhaust the tube, and gradually the luminous discs become fewer and disappear. Finally, the glowing cap of the cathode moves forward leaving a fresh dark space behind, which is known as the "Crookes' dark space." If the air-pump is a good one and the tube perfectly air-tight, the "Crookes' dark space" ultimately fills the whole tube and no light is to be seen, except a glowing phosphorescence on the glass of the tube. This is the state of things that exists in the Röntgen-ray tubes, which probably are not altogether unknown to my readers. It is not the Röntgen rays that now concern us, however, but the state of affairs in the dark space inside the tube.

This dark space possesses some very remarkable properties which can be accounted for by supposing that something invisible is shot off continuously from the cathode: something which flies in straight lines and generates light when it strikes the glass boundary of the containing vessel, or impinges on any solid obstacle placed in the dark space to intercept it. If the exhaustion of the tube is not too high a piece of platinum may be made red hot by this invisible bombardment, but at the highest exhaustions the X or Röntgen rays chiefly are generated. It is found that the bombarding matter flies in straight lines, refusing to turn corners, and can force its way through thin sheets of metal. Indeed, it can actually escape from the vacuum tube into the air if the tube be provided with a window of thin aluminium so placed that the bombardment falls upon it. Finally, these "cathode rays" behave, under the influence of magnets, as if they carried electricity, and they can be shown to possess momentum by making them work little windmills placed in their course. In short, all the facts suggest that the cathode shoots off streams of particles carrying electricity.

The wonderful properties of the dark space in the Crookes' tube were supposed, at one time, to be due to atoms shot off from the cathode by electric propulsion, and moving, like a wind, away from their source; but it was seen that if they were ordinary atoms, then they were ordinary atoms in a very extraordinary state; and Sir William Crookes, struck by the unlikeness of the properties of the dark space to those of ordinary gaseous matter, pointed out that it seemed as if he had obtained matter in a "fourth state," in which it was neither solid, liquid, nor gas.

The solution of the problem presented by these extraordinary phenomena has been worked out chiefly by the Cambridge physicists under the leadership of Professor J. J. Thomson, and it bids fair to revolutionise our ideas on the whole subject of the constitution of matter.

We have learnt that the properties of the Crookes' dark space are such as it would possess if particles of some kind were driven from the cathode which travel at vast velocities and in straight lines, till they meet with some obstacle, such as the sides of the vessel, objects placed in their path, or even the residual air of the tube. But though the radiation flies in straight lines, and refuses to turn corners, its course can be deflected by magnets. And further, the deflection produced by a magnet is such as to show that the charges carried are negative charges, as, indeed, might have been expected, since what carries the charges is expelled from the cathode.

Now, if the cathode rays of the dark space of a Crookes' tube consist of a stream, or wind, of negatively electrified atoms, then, from what has been said, every atom may be expected to carry the same quantity of electricity as a hydrogen atom in electrolysis, or some multiple of that quantity; and if we could determine the mass which carries this quantity of electricity in the dark space, we should know whether the particles are ordinary charged atoms or something different. This is just what has been accomplished by means of a series of researches which not long ago were said "to constitute the present high-water mark of the world's experimental physics."

It would be impossible to describe these researches in detail; therefore it must suffice to say, in the first place, that the cathode rays travel at a rate which is about one-tenth as great as the velocity of light, and thousands of times faster than sound. Secondly, that the mass which carries the above-mentioned unit of electricity is only about the one seven-hundredth part as great as the mass of an atom of hydrogen. It makes no difference whether the tube is filled originally with air, oxygen, nitrogen, or some other gas, in every case the mass carrying the so-called "atom of electricity" is of the same order and very greatly less than the mass of an atom of hydrogen. These facts do not seem consistent with the idea that ordinary chemical atoms are the carriers of electricity in the cathode rays, for they do not correspond with what we believe to occur in electrolytic changes.

We can account for these phenomena by two equally simple hypotheses. We may suppose that the flying particles of the cathode rays are ordinary atoms carrying electric charges vastly bigger than those we meet with in electrolysis. Or we may suppose that we have to deal with particles whose masses are only about the one seven-hundredth as great as those of hydrogen atoms, and that each of these carries an electric charge equal to that carried by the hydrogen atom. Now there is this fact in support of the second hypothesis: that whilst the speed of the particles is enormous, their energy is not great. And as the energy of a moving body depends on its mass and its velocity, it seems reasonable to suppose that particles moving with vast velocities, but endowed with only moderate amounts of energy, are of very small mass. This kind of reasoning is not conclusive, however, and Professor Thomson has shown us that the way to get a final answer to the question is to count the particles in a given space, and to determine the total quantity of electricity carried by this collection of particles. These being known, the charge carried by each of the particles can be calculated. Just imagine the character of the task. Every cubic centimetre—say, one-third of a thimbleful—of a gas contains 20,000,000,000,000,000,000 molecules, and in this case the number of particles might be seven hundred times greater. It was managed in this way:

One of Dr. Thomson's colleagues, Professor C. T. R. Wilson, has shown that when electrified particles are present in moist air quite free from dust,[1] a mist is produced if the air is suddenly expanded, water condensing upon the particles so that each becomes the nucleus of a microscopically small drop of water. No such mist will form if the air be perfectly free from these electrified particles.

Again, the late Sir George Stokes has shown that we can calculate the rate at which a drop of water will fall through the air if we know its size, and therefore it was possible for Professor Thomson to find the size of the drops forming a mist by measuring the rate at which the mist subsided. Then, knowing the average volume of each drop, and the whole volume of the water which had gone to form the drops, he could calculate the number of drops by dividing the total volume of the water by the average volume of a drop. But this gave him the number of electrified particles, for each of these formed the nucleus of a drop. Finally, having measured the total electric charge carried by the particles in the given space, and knowing the number of the particles, he was able to calculate the charge carried by each. The result arrived at was that each particle carries a charge which approximates to that carried by an atom of hydrogen in electrolysis. Therefore, in cathode rays we have particles far smaller than atoms of hydrogen which carry charges equal to those which have been termed "atoms of electricity."

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[1] Mr. J. Aitken of Edinburgh discovered in 1880 that a mist will not form in the moist dust-free air. This fact has been made the basis of a method of counting the dust particles in the air.

The ingenuity of the counting process which enabled Professor Thomson to make this discovery will be better appreciated when I say that in one experiment he found 30,000 droplets in each cubic centimetre of air, and that their total weight was about the thirteen-thousandth part of a grain. The exact masses of these particles, or "electrons," as we may now call them, are, of course, not known. We may take it, however, that the mass of an atom of hydrogen is about seven hundred times as great as that of an "electron."

It may be mentioned that electrons do not occur only in the Crookes' vacuum tube. For example, they are given off by metals when heated to the highest temperatures, and when illuminated by ultra-violet light, and also by Radium. They seem to be concerned always in the transport of negative electricity at low pressures.

Summarising, the conclusions reached amount to this. It is wrong to suppose that the hydrogen atom is the smallest particle. On the contrary, there exist in the cathode rays, and elsewhere, negatively electrified particles whose masses are far smaller than those of hydrogen atoms. These particles can travel great distances, when not impeded, at vast velocities. When they strike massive obstacles they are stopped, and then generate the X-rays of Röntgen. They can heat platinum to redness and drive small windmills, can pass through thin sheets of aluminium, and thus can escape from air-tight vessels. They act on photographic plates and cause phosphorescent solids to glow. Air containing them becomes a conductor, and they discharge positively electrified bodies, which affords us a means of detecting them. The Physicist asks us to admit that the atoms of the chemist can no longer be regarded as indestructible. On the contrary he tells us that we can knock at least one electron off every atom.

But we are not yet at the end; we do not know, for example, what the constitution of the electrons may be, whether they are partly material and carry charges of electricity, or whether they are simply charges of electricity and nothing else. For according to the teachings of a modern physicist this latter view has the advantage of being the simpler, and by no means need be rejected summarily. Even if we adopt the second and simpler view of the nature of an electron, we must not jump to the conclusion that all matter is made up of these minute negatively charged particles. For it seems that a very different set of facts has to be dealt with when we come to consider positive electricity. The masses of the particles which carry positive charges have also been measured, and it is found in their case that the unit quantity of electricity is associated with masses of the same order as those of ordinary atoms, and that these vary according to the nature of the gas in which they occur—a fact which has led to the suggestion that possibly "negative electricity may consist in the minute corpuscles, and that positive electrification consists in the absence of these corpuscles from ordinary atoms." This is a view which, as Professor Thomson points out, brings us back very close to the one fluid theory of electricity propounded long ago by Benjamin Franklin. Sir Oliver Lodge in his "Romanes Lecture," pressing the electric theory of matter to the furthest point, suggests on the other hand that it becomes reasonable to suppose "that the whole atom may be built up of positive and negative electrons," these electrons being "interleaved or interlocked in a state of violent motion so as to produce a stable configuration under the influence of their centrifugal inertia and their electric forces" ; a charged atom, or an ion, being one which has one electron in excess or defect. A view of the matter which brings into our minds the famous hypothesis of "Prout," that all the elements are composed of the same fundamental substance,[1] but which does not merely reassert "Prout's" hypothesis in its original form, since we must now contemplate the possibility that the primordial substance may be electricity and nothing else. The weak point of these latter speculations lies, as Sir Oliver Lodge has pointed out, in the fact that as yet no one has isolated a positive electron; positive electricity having been met with, so far, only when associated with ordinary atoms such as those of hydrogen.

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[1] Prout suggested that hydrogen was the fundamental substance.

It is impossible within the available limits to pursue the speculations which find in electrons the basis of all electrical and many other phenomena, such, for example, as those which find in their motions the source of light itself. But I may mention in passing an explanation of the cause of that most beautiful mystery the Aurora Borealis which we owe to Arrhenius and others.

As very hot metals emit electrons, we must admit that an intensely hot body like the sun is likely to throw them off in vast numbers, and, as a consequence of this, we must admit further that showers of electrons after travelling through the interplanetary space will fall upon the upper layers of our atmosphere. But the earth is a magnet, and therefore the electrons that reach our atmosphere will come under the influence of its magnetic qualities. Now, it is found that owing to the distribution of the lines of the earth's magnetic force those electrons which reach the outer layers of the earth's atmosphere near the equator will not penetrate far, but will travel horizontally, and therefore will stay at the higher levels where the density of the air is extremely low, and where but little luminosity can be produced by their passage through the gas. But at higher latitudes, owing to a different distribution of the earth's magnetic force, the electrons, following the lines of force as they dip, will rush downwards into the lower and denser levels of the atmosphere, and there produce the luminous phenomena of the vacuum tube. That is, according to this hypothesis, the Aurora Borealis.

A little while ago this, or something like it, would have told the whole of my story. But not so to-day. Quite recently the results of a series of wonderful researches bearing upon the constitution of matter have illuminated the subject as by a lightning flash, giving us a glimpse into the properties of matter, such as no one could have hoped for a few years ago.

We all know that for many generations men believed in the possibility of transmuting the metals. Nay, that not a few investigators wasted their lives in foolish attempts to produce gold from the baser metals. We know too that men of wide learning and sound sense, like Roger

Bacon and others, believed transmutation to be practicable, and that though few such persons professed to have actually carried out a transmutation, not a few believed that they had seen it done, or at least that they had witnessed the "philosopher's stone" by the aid of which it might be accomplished. It was only in the sixteenth century, after Paracelsus had directed attention to the importance of chemistry to medicine, that the hunt for the philosopher's stone began to slacken rapidly. Its death-knell was rung, as it seemed at the time, two centuries later when Lavoisier taught us to regard the metals as elements. Of late years, however, there has been a less widespread disposition to pour scorn upon the heads of the alchemists than during the period which succeeded the discoveries of Lavoisier. We have been less indisposed than we used to be to admit that some day some element might be transformed into another. Still, even after the researches aud speculations described above had weakened our conviction that, practically speaking, atoms are indivisible, I suppose few thought it at all probable that the subject of transmutation would come up again for serious discussion in our own times. And yet the most sceptical must admit that we are hardly clear of such a discussion at the present moment.

Every one knows that in 1896 M. H. Becquerel discovered a new property of uranium, which in the hands of Mme. Curie led to the discovery of radium, an element which glows in the dark, appears to give off energy without ceasing, and can maintain itself at a temperature about 1.5° C. higher than that of the things around it. Naturally the remarkable qualities of this astonishing element have been the subject of much curious scrutiny.

When the unique properties of radium and the other radio-active bodies were first discovered they were thought to be due to the generation of a kind of Röntgen rays. Then they were attributed chiefly to the emission of electrons, like those of the Crookes' tube, except that they move with much greater velocity, and further experience shows that both these are indeed given off. But it now appears: first, that by far the most important part of the radiation of radium consists of much larger masses than electrons, in fact of particles as large as chemical atoms. These heavy radiations are positively electrified, they move with great velocities, are endowed with great amounts of energy; they make the air through which they pass conduct electricity, and reveal themselves to the eye by the flashes they produce when they bombard a suitable screen; flashes which may be seen if they are examined under a lens, as in the little instrument invented for the purpose by Sir William Crookes and called the "spinthariscope." Secondly, and this is the most important of all, that we must seek the solution of the "Mystery of Radium" not only in the Röntgen rays, nor in the electrons, nor in those heavier particles which are the source of the greater part of its radiant energy, nor in all these taken together, but also by studying the matter which these leave behind them—by studying, for example, certain products known under the names Thorium X and Uranium X,[1] and above all by investigating the "emanations" of radium, thorium and the other radio-active forms of matter. These emanations, which must not be confused with the radiations mentioned above, are in every respect like ordinary matter. They are volatile, they mix with other gases, can be bubbled through liquids, can be condensed by great cold and are re-volatilised by heat. They are often occluded in the substances which produce them, and when that is the case they can be liberated by dissolving the substances in water. They differ from ordinary matter in one respect, viz., they are found in such minute quantities that they cannot be detected by the most delicate scales and weights. They even defy the powers of the spectroscope. But in spite of this the researches of Professor Rutherford and others have placed their material existence beyond all doubt.

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[1] Other substances exhibit radio-activity as well as radium, *e.g.* thorium, but for the sake of simplicity I confine my remarks chiefly to he former.

It seems clear that radio-active matter, such as radium and thorium, is matter in a state of change—matter, that is to say, which is undergoing chemical change, since new substances are being formed at every stage. Moreover, the facts of the case are inconsistent with the idea that the change is of the nature of a combination. On the contrary, it seems to present the characters of a decomposition. According to Professor Rutherford, the change which accompanies radioactivity consists in the gradual disintegration of the radio-active substances in such a way that they give off, besides electrons, charged particles whose masses are of the same order as those of atoms of hydrogen, whilst new forms of matter, lighter than the original substance, are left behind. These residues, which also are radio-active, are not the ultimate products of the change, however, but themselves undergo further change, so that the process goes on continuously, stage after stage, each stage being accompanied by radio-activity, such as that of radium.[1]

A few years ago, our fellow-countryman Newlands and the great Russian chemist Mendeléeff, after comparing the properties of the chemical atoms with their respective weights, came to the conclusion that there is a distinct connection between them of such a kind that the atomic weight of an element being known, it becomes possible to foretell its properties: to say, for example, whether the element is metallic or non-metallic, gaseous or solid, at ordinary temperatures, and so on. And the latter, having arranged all the elements then known in a table according to the law of periodicity, as it is called, went so far as to assert the existence of several elements which had never been met with, and to foretell their leading properties. Soon after this, two new elements, gallium and germanium, were isolated, and when their properties had been ascertained, it was found that they were two of the hypothetical elements discovered, so to speak, by Mendeléeff by means of the periodic law, which of course strengthened the belief of chemists in the validity of the law.

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[1] So much has been said about the apparently inexhaustible character of the radio-activity of radium, that it is interesting to learn that Professor Rutherford estimates the "life" of a given specimen of radium at only a few thousand years.

The difficulties and pitfalls in the way of an investigator who has to deal with new forms of matter in quantities so minute that they are beyond the reach of the spectroscope are so great, that the results even of the most brilliant experiments made under these circumstances are sure to be received with a certain amount of reserve. Hence it is interesting to find that Professor Rutherford's conclusions soon received support not unlike that brought to the originators of the Periodic Law by the discovery of gallium and germanium.

It is natural to speculate on the probable nature of the ultimate product or products of these newly discovered transmutations. One thing seems probable. The ultimate products of these changes will not be radio-active like the emanations we have been considering. Hence they may prove to be elements already known to us, and Professor Rutherford and Mr. Soddy suggested at an early stage that it seemed not improbable that helium might be one of them. This prophecy, like that of Mendeléeff, was very quickly confirmed, by Sir William Ramsay and Mr. Soddy, who discovered, a year or two ago, that if the gases obtained from a solution of radium bromide in water be freed from oxygen and hydrogen, dried by phosphoric oxide, and then cooled with liquid air, to remove carbonic acid gas and the emanation, they give in a vacuum tube the spectrum of the element helium. Nor is this helium a mere accidental impurity, for it is found that though no helium can be detected in the freshly formed emanation of radium, it can be found there after four days' standing.

Fuller details of the story of the disintegrations of radio-active substances will be found in the two essays on Radium, but enough has been said, perhaps, to show the bearing of these disintegrations on the subject of the "Constitution of Matter." In the first place, the fact that radium and its allies disintegrate spontaneously and continuously, emitting electrons like those met with in the Crookes' vacuum tube, cannot fail to stimulate those workers who suggest that matter consists of positive and negative electrons interlocked and in a state of violent motion. Secondly, they raise the question: How can we account for the transformation of radium into helium, without admitting that here, to-day, in the twentieth century, we have a change which strangely resembles the transmutations our ancestors sought to bring about in the Middle Ages? The answer to this question must be looked for in a subsequent essay, but this much may be said here. Either the disintegration of radium and its fellows are transmutations such as Bacon dreamed of, or we have discovered in uranium, radium, and the other radio-active substances, new compounds of a type so novel and so unexpected that the fact of their existence is hardly less important than the performance of a true transmutation.

SOME RECENT THEORIES OF THE ETHER

I THINK I can scarcely contrive a more fitting introduction to an essay on "the ether" than the two quotations which follow. They indicate in the fewest possible words how far we have travelled since the days when "the ether" was invented by Huygens, for the simple purpose of accounting for the propagation of light.

The first of these quotations is taken from the late Professor Preston's book on Light, and it runs as follows: "The present tendency indeed of physical science is to regard all the phenomena of Nature, and even matter itself, as manifestations of energy stored in the 'ether.'" The second comes from a "Silliman Lecture," delivered at Yale University, by Professor J. J. Thomson, about three years ago. On one view of the constitution of matter, said Professor Thomson, "All mass is mass of the ether, all momentum, momentum of the ether, and all kinetic energy, kinetic energy of the ether." If these two extracts truly represent the present trend of physical speculation, could any scientific topic be more important or more interesting?

Only as recently as the year 1894, when he was President of the British Association at its last meeting at Oxford, the late Marquis of Salisbury reminded the assembled parliament of science that at present we know absolutely nothing about this all-pervading entity, the ether, except this one fact—that it can be made to undulate, and that it performs even this solitary function in an abnormal fashion which has caused infinite perplexity. It is my object to tell something about the present state of our knowledge of this elusive entity, and to indicate, as far as I may, the lines followed by some recent speculations concerning its nature and its relations to those other manifestations named by us matter and electricity.

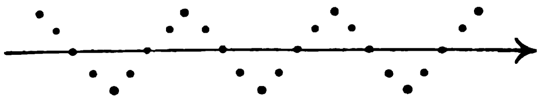
First, let us consider how it has come about that this hypothetical medium called, or I should say recalled, into existence a century ago by Dr. Thomas Young for the single purpose of explaining the phenomena of light, now plays so dominating a part as that assigned it in the two passages quoted above.

I need hardly remind my readers that the notion that there exists an invisible intangible material, filling all that part of space not occupied by ordinary matter, is one of the oldest in science. But many of them may not know that, at one time, ethers were created by men of science almost as plentifully as blackberries by a blackberry bush, that they were called into existence in every difficulty with reckless profusion. Ethers have been invented, as Clerk Maxwell has said, "for the planets to swim in, to constitute electric atmospheres and magnetic effluvia, to convey sensations from one part of our body to another, till all space was filled several times over with ethers," with the result that science in the end turned restive under this "multiplication of entities," this constant piling up, so to speak, of the ethereal population of space and, after a period of reaction during which it became almost a point of honour to discard the assistance of ethers, now contents itself with a single ether—viz., that invented by Huygens in 1690—to explain the propagation of light. But this single ether, as we shall see, has to carry a heavy burden and to perform many and sometimes incongruous functions. It is, as Miss Agnes Clerke has wittily remarked, at once the universal provider and the universal messenger. It is Atlas and Mercury rolled into one.

It may be said, I think, in a general way, that our single ether owes its survival to the unwillingness of men of science to admit the possibility of "action at a distance," their unwillingness to admit, for example, that gravity is a primary property of masses, incapable of explanation, and acting at all distances across empty space; for it followed from this that when the undulatory theory of light was established by Young and Fresnel in the early part of the last century, the conception of a luminiferous ether was accepted as a necessary part of the theory. How could waves of light and heat emitted, for example, by the sun reach the earth unless some medium capable of undulating occupied the interstellar space between them? For if waves travel from the sun to the earth, then is it not evident that these waves must be waves of something or waves in something? Or, to look at the matter from another point of view, if light be a manifestation of energy, which is *ex hypothesi* indestructible, and if it be not carried to us by minute particles, as Newton supposed, then what becomes of it during the eight minutes which elapse between the moment when it leaves the sun and that at which it reaches the earth's atmosphere? Where is it stored during those eight minutes when it is neither on the sun nor on the earth? The answer to these questions is this: The missing energy is in the ether, and the propagation of light across the interstellar space, and anywhere and everywhere, depends upon waves in this ether, which fills all space and permeates all matter.

Most of us will agree that, if we accept the undulatory theory of light, we are bound to admit the existence of some medium such as the ether. But when we attempt to form a mental picture of this ether, even if we neglect for the moment all its properties except its optical properties, we find ourselves in difficulties, for none of us have ever met with anything like it before. It cannot be a gas, for light passes through an exhausted vessel, and through the interstellar void, which we believe contains no gas; and for similar reasons, still less can it be regarded as a liquid or a solid, though it must be incompressible and resist cutting even more strongly than steel itself. One thing, as Lord Salisbury said, we do know about the ether. If it exists at all, it can undulate. We feel we tread solid ground here, for if the ether could not undulate, then it could not transmit the vibrations which we call light.

The ethereal undulations which constitute light must differ widely from the motions which originate the waves of the sea, or the aerial disturbances known as sound, and the elasticity of the ether must be of a different order from that of the familiar gases, liquids, and solids. Air yields to pressure, and sound depends upon oscillations of air particles backwards and forwards along the line of propagation of the audible disturbances. The ether, on the other hand, must be regarded as incompressible; for the properties of light require us to assume that light-waves are not produced by the compression and rarefaction of a medium like the air, that they are not waves such as might be produced, for example, if the separate type on this page should begin to oscillate backwards and forwards, from left to right, and right to left, along the lines of print, but transverse waves such as we should have before us if the type were to swing upwards and downwards across the lines so as to produce more or less the effect suggested by the following diagram:



It is difficult to picture a substance which we cannot weigh, which is as rigid as steel to pressure, and yet yields, to some extent, to the distortional stresses that will account for the propagation in it of luminous undulations. Nor are our difficulties diminished when we remember that we must conceive this bewildering entity as filling all space, permeating the inmost recesses of all matter, solid, liquid, and gaseous (for in its absence how could these transmit light and other electromagnetic disturbances?), rigid, as I have said, as steel, and yet permitting material particles like grains of sand or the earth to move freely through it. Clearly the most learned of us has no experience to appeal to here. How can we draw a mental picture of such stuff as this? Think of men blind from their birth groping their way through a world they have never seen, and you will have some conception of the difficulties which stand in our way. But on fuller thought you will see also that the problem may not be altogether beyond our powers. The blind man with his stick learns much that is true about the world he lives in—enough, in fact, to enable him to construct in his mind a useful, if imperfect, hypothesis or working model of his invisible environment; and so, similarly, with the resources at our command, limited though they may be, why should we not discover a great deal about this ether which we can neither see nor feel, but which exists, as we are convinced, in us and around us? The picture we may form, like a blind man's model of his unseen world, may be, at first, but an imperfect and colourless reproduction of the reality. But what of that? It will grow more true and more perfect, and in time may even gain something corresponding to colour, if we press on.

But whilst the task of forming a clear idea or mental picture of the ether constitutes one of the most difficult labours allotted to science, even when we consider only its function of propagating the transverse vibrations which constitute light, the difficulty of that problem is increased a hundredfold when we burden our medium, in addition, with the duty of transmitting the pull of gravity from particle to particle, and from world to world, and seek to evolve from it matter in its myriad phases, and electrons, that is electricity—if, indeed, matter and electricity really are distinct, and not merely two different phases of a single primary material, viz. "the ether" itself.

Needless to say, recent speculations on the subject of the ether do not all start from a common point. Two of the chief of these attempts to sound the depths deal with the matter from the point of view of the engineer or physicist, while in a third case the picture is drawn for us by the eminent Russian chemist, Professor Mendeléeff. The authors of the former, starting with the functions of the ether, endeavour to give us pictures or models, more or less precise, of a medium which might conceivably execute those functions. Professor Mendeléeff, on the other hand, starting with the eighty elements known to the chemist, attempts to work backwards from these to a mind-picture of the ether. Let us first go over the ground in company with the chemist, and learn what he has to tell us.

Mendeléeff takes for his starting-point the great periodic law of chemistry, which he did so much to establish, and the modern discovery of the inactive gases of the atmosphere, argon, helium, neon, xenon, and krypton. He asks us to believe that the ether is an extremely inactive and extremely attenuated gas, and then attempts to apply the periodic law to the discovery of this gas, or rather to the purpose of proving that such a gas exists, or may exist, throughout the universe.

According to the periodic law of Newlands and Mendeléeff, the properties of the chemical atoms vary periodically with their weights in such a manner that if we arrange the elements in the order of these weights, we shall find that similar variations in their properties recur at more or less regular intervals. Thus, if we write down in this order lithium and the thirteen elements which follow it, viz. :—

Lithium.	Beryllium.	Boron.	Carbon.	Nitrogen.	Oxygen.	Fluorine.
7.0	9.0	11.0	12.0	14.0	16.0	19.0
Sodium.	Magnesium.	Aluminium.	Silicon.	Phosphorus.	Sulphur.	Chlorine.
23.0	24.0	27.0	28.0	31.0	32.0	35.0

we find they form seven successive pairs, each pair so much alike that if we know the properties of one member of any pair and the properties of its compounds, we can state with considerable certainty the character of the second member, and forecast the characters of its compounds with other elements. Thus, for example, if we know that sodium, the second element of the first pair, is a metal which decomposes cold water, setting free hydrogen, that its salts are soluble and its oxide strongly alkaline, caustic and able to form a soap when heated with oil, we may reasonably conclude that lithium also is a metal which will act more or less strongly on cold water, that its salts also will be soluble, and its oxide, like that of sodium, alkaline, caustic and able to saponify oils and fats. Now, the whole of the elements can be arranged in a number of series like those given above with more or less satisfactory results, but though the two series I have selected to illustrate the law are, as it happens, complete, this is not true of every series, and when Mendeléeff drew up his first periodic table of the elements he found it necessary to leave many blank spaces in that table. In fact, these blank spaces were so numerous that he might very well have concluded that the whole system was wrong. Fortunately, he did not do this, but, on the contrary, perceived that the occurrence of such spaces was to be expected, that it was not likely that chemists had as yet become aware of all the elements in the universe or even on the earth, and that these vacant places might well be supposed to correspond to elements which exist somewhere and might yet be discovered. Indeed, he went much further than this. On the strength of his opinion he deduced the properties and atomic weights of some of the missing elements. And with most triumphant results. In a few years, when the three new elements gallium, scandium, and germanium were discovered, each was found to fill a vacant place in Mendeléeff's table, each had an atomic weight corresponding to that of one of the missing elements, and each had the properties which Mendeléeff had foreseen and foretold as likely to be exhibited by the element having that particular atomic weight. Thus the periodic law became firmly established. It not only co-ordinated the known elements, it afforded in addition a simple and trustworthy means of foretelling the existence of others still unknown.

But though the periodic law could be employed in predicting the existence of many unknown elements, it did not, and in fact could not, enable Mendeléeff to foretell the existence of argon, helium, and their companions, for no inactive element like these was known when the law was first enunciated. Therefore, when these were discovered it became necessary to extend the table drawn up by the great Russian chemist by adding to it a new group. This addition at once placed him in a position to predict the existence of elements of the inactive or argon type, and enabled him to form his chemical conception of the ether.

The element which has the lightest atom known to us, if for the present we exclude the coronium of the sun's corona, is hydrogen. That which has the heaviest atom is uranium, and the respective atomic weights of these elements are approximately as 1 to 240.[1] When Mendeléeff first used the periodic law as a means of predicting the existence of certain unknown elements, he followed what mathematicians call the method of interpolation ; that is, he inserted the predicted new elements among the rest, at points where obvious gaps occurred, deducing their properties from those of the elements around them. Accordingly all the elements thus predicted fell within the limits mentioned above: none of them had atoms lighter than hydrogen atoms, and none had atoms heavier than those of uranium. In the case of the ether, however, the method of prediction was of necessity different. There can be no doubt, if we admit that the hypothesis is in any degree valid, that the ether of Mendeléeff must consist of particles vastly smaller than those of the lightest gas, and far smaller, again, than even the electrons of the Crookes' vacuum tube, which are a thousand times smaller than atoms of hydrogen ; for the ether must not only be able to penetrate solids slightly as hydrogen does, or somewhat freely like the electrons, but to an extent far transcending anything we can imagine from our experience with these. Therefore it was impossible to adopt the method of interpolation in the case of the ether, for no place could be found in Mendeléeff's table for an element having atoms thousands and thousands of times lighter than hydrogen atoms. In fact, it was necessary to extrapolate the ether ; to venture, that is, outside the limits of the periodic law into regions beyond the range of actual experience. The results of Mendeléeff's excursion into these regions are as follows: First, he predicts the existence of a new inactive element whose atoms are not more than four-tenths as heavy as those of hydrogen. This, as he foresees, very possibly may be coronium, whose spectrum is visible in the solar corona, which is suspected to occur on this earth among the gases belched out by volcanoes. And secondly, after inventing a new series which, however, includes at present no known element, he infers the existence of yet another new element, *x*. This he considers to be the lightest of the elements, the most mobile gas, the element least apt of all to combine with others, and in addition an all-permeating and penetrating substance. Here then is Mendeléeff's theory of the ether. In a sentence, the ether is an inactive element having excessively light atoms. That such an element may exist does not seem altogether inconceivable, and chemists will admire the ingenious process by which the great master has secured this aftermath of the periodic law. But whether this mobile element can be shown to constitute the ether is another question.

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[1] This means that an atom of uranium weighs as much as 240 atoms of hydrogen.

Though the data afforded by the table of known atomic weights enabled Mendeléeff to calculate the weight of the atom of the heavier of the two new elements thus predicted, since its value carries us but little beyond the range of actual experience, they are not equally helpful in the case of x. In fact, it must be admitted that there is not yet much real evidence of the existence of x, and the hypothesis, so pleasing to the chemist, must be declared to be not proven; nor, indeed, does its distinguished author do much more than put it forward as a suggestion which deserves to be considered. It is not, I believe, denied that the phenomena of light might, given certain conditions, be accounted for upon an hypothesis of this order. But before such an ether as that of Mendeléeff can be accepted, it must be established that the particles which compose it move, broadly speaking, with the velocity of light in every direction, that their free paths are of such vast length that collisions among them can never occur, that it is possible for vibrating bodies to impress upon them some property, such as rotation about an axis, which shall not interfere with their motion of translation; and, lastly, that light shall be shown to consist in the alternation of the average value of the property in question.

Now let us turn to the pictures drawn by the physicists.

Of all the theories of the ether, that lately propounded by Professor Osborne Reynolds is perhaps the most startling. It inverts all our previous ideas on the subject. According to this, the youngest theory of the ether, we must look upon the ether as the one really substantial thing in the universe, its density being ten thousand times greater than that of water, while matter, which seems so substantial, consists, so to speak, in an absence of mass and has the character of a mere wave in the ether. On this newest view "we are all waves," as the author of the theory, bursting into poetry, exclaimed at the close of the eighth section of his Rede lecture. This astonishing hypothesis cost its author no less than twenty years of labour. It asks us to imagine that the universe, except those minor portions which constitute matter, is built up, like a bag of sand, of grains of definite shape and in size so inconceivably small that their diameters are no greater than the seven hundred thousand millionth part of the wave-length of violet light, which in its turn amounts to only sixteen millionths of an inch, and so closely packed that, though not absolutely immovable, the four hundred thousand millionth of the seven hundred thousand millionth of one sixty thousandth part of an inch—*i.e.* the four hundred thousand millionth part of their own diameter — would represent approximately the mean free path through which these particles are free to move. Professor Reynolds tells us that the density of this medium, far from being almost indefinitely small, is nearly five hundred times as great as that of the densest matter known to us on earth, and its pressure more than three thousand times greater than that which any material yet tried has been known to sustain.

To get some idea of this conception of the ether, picture to yourself a billiard table carefully packed from one end to the other with line after line of billiard balls, each line so nicely fitted or geared into the next that the balls are packed almost as close to each other as is possible, yet not so very tightly as to prevent, absolutely, all motion among them. Imagine, again, that you have not one layer of balls, as on a billiard table, confined by the sides of the table, but layer upon layer piled one above the other and extending absolutely without limit in every direction. Remember that these balls or grains are so minute that, say 11,200,000,000,000,000,000 of them laid side by side along a line would only occupy a single inch, and you will have a picture, so far as may be, of Professor Reynolds' conception of the universal medium, the ether.

It is a peculiar fact, which can be illustrated practically by means of some small shot in an indiarubber bag, that such a system of particles as that which I have just described does not contract when submitted to pressure, but, on the contrary, undergoes expansion, and for the following reason. When a system of hard grains arranged in "normal piling," as Professor Reynolds calls the state of affairs described above, is disturbed by pressure, the particles must of necessity move not towards one another, if they move at all, but, on the contrary, away from one another, since to begin with they are packed as close together, practically speaking, as they can be. When such a system is submitted to a strain the gearing of the particles is affected, layers of particles being thrown out of gearing here and there, whereby certain "singular surfaces" or lines of misfit are formed in the system.[1] These lines of misfit can be propagated indefinitely in any direction, and when being so propagated they constitute, on this hypothesis, matter in motion. Thus, as I said before, according to this astonishing conception, the ether alone has any concrete existence in the universe, and matter consists of mere waves or eddies passing through it. We have all watched the golden ears of corn waving in the wind on summer days. With this remembrance to help, you may perhaps be able to picture broadly such a state of things as that which I have endeavoured to depict. Imagine undulations, due to the translation of "lines of misfit," moving eternally in every direction through a universe of closely packed particles. Then these waves, due to the propagation of singular surfaces, constitute matter on Professor Osborne Reynolds' hypothesis. In them he sees the molecules of the chemist, and by means of his theory he claims to account for such phenomena as electricity, gravitation, and the propagation of light. In thinking of matter from this point of view we must not forget that motion is as real as matter; that the waves which play over the corn are not less real than the corn itself, otherwise we may do the theory and its distinguished author less than justice.

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[1] To see what is meant by a line of misfit you may make an experiment with some marbles in a plate, first gearing them together all through the mass, and then throwing two lines out of gearing by pressing a strip of cardboard or thin sheet-metal between them.

We owe the last new theory of the ether, which space permits us to dwell upon, to Dr. Larmor, of the University of Cambridge. This has but little in common with the hypothesis we have just considered, except that Dr. Larmor, like his colleague, seems to regard the ether as the concrete reality, and asks us to look upon matter as so comparatively intangible and unsubstantial in character that a friendly critic, after perusing his recent book on "Æther and Matter," remarked that he presumed its title was the result of a typographical error, and must have been written originally "Æther and no Matter." Dr. Larmor is a leader in that distinguished school of physicists which is disposed to consider it likely that the chemical atoms are built up "of positive and negative electrons interleaved or interlocked in a state of violent motion so as to produce a stable configuration under the influence of their centrifugal inertia and their electric forces." Hence the electric theory of matter, as might have been expected, plays a leading part in this attempt to penetrate the secret of the ether.

A few years ago Lord Kelvin charmed and delighted the world with a conception of matter which pictured it as consisting of "vortex rings" formed in a perfectly frictionless fluid—the ether.

It is rather difficult at first to imagine conditions under which flexible chains or indiarubber tubes filled with water could become rigid, without the links of the chains being bolted together or the water frozen ; and yet there are circumstances under which this occurs. For example, if you join the two ends of a common watch-chain, and then by means of pulleys set the chain rotating rapidly, it will become stiff, and presently, if the rate of rotation be raised sufficiently, will be found to retain its rigidity so completely when the pulleys are withdrawn that it might easily be mistaken for a ring of solid metal. And, again, although a circular india-rubber tube filled with water is limp and flexible, the same tube becomes so stiff when its liquid contents are made to rotate vigorously that it will stand upright. The following experiment illustrates the point still better.

Get a wooden box having sides about 24 inches square—a common sugar-box will do—replace its lid closely by a piece of cotton cloth fixed securely round the edges. Cut a sharp-edged circular hole 4 or 5 inches in diameter in the centre of that side of the box which faces the cloth. Place inside the box a small dish containing some very strong solution of ammonia, together with a second dish containing either some very strong spirit of salts gently warmed, or a mixture of oil of vitriol and common salt, and let them stand till the box is filled with a fine white smoke of ammonium chloride. Then place this apparatus at one end of a long room, and convey a series of sharp impulses to the air within the box by withdrawing the cloth covering, and suddenly pushing it back into the box. When you do this, a magnificent smoke-ring will sail across the room after each operation. These rings consist for the most part of air, but are made visible by the opaque particles of ammonium chloride mingled with the latter. They will blow out a candle placed several feet away from their point of origin, and probably will retain their form till they impinge on some solid object, such as the wall of the room. In short, these rings of rotating air possess some considerable degree of rigidity. They will even bear blows, in moderation, as you may prove by sending one such ring quickly after another which is travelling at a slower rate, when you will see that as they approach, and still more when they come into contact, each visibly affects the other much as two solid objects might do. Look at some of these smoke-rings closely, and you will soon be satisfied that the air composing them is in circular motion—that the rings are built up, as it were, of a number of "vortex stream lines" more or less resembling the rotating chain described above.

The rigidity of a rotating chain rapidly diminishes and soon disappears if we stop the machine which drives it, and, similarly, though a smoke-ring may travel a good many feet in still air, yet after a while it gradually falls to pieces before our eyes. This is due to friction among the rotating parts of the system. The energy of the system is gradually frittered away as heat, the motion diminishes, the ring gradually loses its rigidity, and presently its component particles are once more indistinguishable from those of the surrounding air. But imagine vortex rings set up in a perfectly frictionless fluid. Would not these be eternal? Or suppose the fluid to be only a very near approximation to a frictionless fluid. Then would they not, at any rate, seem eternal? In short, does it not appear conceivable that the atoms of the chemist may be vortex rings formed in the ether? This was Lord Kelvin's theory, of which it has been said that it is so beautiful that whether it be true or whether it be untrue, at least it deserves to be true. On this view, the atoms of the chemist, atoms of radium, atoms of oxygen, atoms of argon, helium, and all the rest of the elements, are not detached particles of alien matter bedded in the ether, like plums in a pudding, but differentiated portions of the ether itself.

Lord Kelvin's theory accounts for much. With its aid we begin to understand, or to feel as if we understand, the indestructibility of atoms and their capacity for definite vibrations which is revealed to us by the spectroscope. Further, this theory gratifies in a striking manner the strong sentiment in favour of a simple universe which has been the source of so many attempts to unify our conceptions of the physical basis of the latter. But it fails to include an electric charge as part of the constitution of the subatoms of matter, and thus on this ground, apart from other difficulties,[1] it fails to satisfy the demands of at least one great school of physicists.

A satisfactory theory of the ether, as Dr. Larmor has pointed out, must account for the conveyance of electric attraction across the ether by elastic action, and an electric field must be a field of strain. Hence each sub-atom, with its permanent electric charge, must be surrounded by a field of permanent strain in the ether. This condition requires us to reject hypotheses based upon the conception of a perfectly fluid ether, and forces us to regard the ether as endowed with some quality of the nature of elasticity. "A protion," or sub-atom of matter, therefore, Dr. Larmor tells us, "must be in whole or in part a nucleus of intrinsic strain in the ether, a place at which the continuity of the medium has been broken and cemented together again (to use a crude but effective image) without accurately fitting the parts, so that there is a residual strain all round the place." The ultimate element of material constitution becomes, on this view, an electric charge or nucleus of permanent strain in the ether instead of a vortex ring generated out of a perfect fluid, as in the hypothesis last discussed, and we may venture to look upon molecules as composed of systems of electrically positive and negative protions in a state of steady orbital motion round about each other. In short, as Dr. Larmor says in "Æther and Matter," it seems as if the master key to a complete unravelling of the general dynamical and physical relations of matter may lie in the fact that it is constituted of discrete molecules "involving in their constitutions orbital systems of electrons, and moving through the practically stagnant ether." I am afraid this will hardly make the matter clear to all; still, perhaps most of us will gather in a general way that according to this view the ether is not a perfectly frictionless fluid, but, on the contrary, is endowed with an elastic quality; that, somehow, electrons, which are minute charged particles of matter carrying electricity or perhaps particles of electricity alone, are generated in this ether, and that systems consisting of electrons revolving round about one another—may we say more or less like the systems of the heavens?—form the atoms and molecules of which the familiar forms of matter are built up. And this must suffice, as it would be impossible in a short article to develop Dr. Larmor's argument fully, or to give even a sketch of the specification of an ideal medium by which he illustrates his conception of the ether as "a perfect fluid endowed with the rotational elasticity demanded by its more obvious properties."

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[1] *E.g.*, vortex rings at a moderate distance from each other will not exhibit in their behaviour to one another anything of the nature of gravitation.

Here, then, we have the outlines of three pictures of the eternal ether, each presenting it as it shapes itself in the mind of a great contemporary thinker. To Mendeléeff, the chemist, it appears to be the lightest of gases, the most inactive of all the elements. Professor Osborne Reynolds, the engineer, pictures it as a mass of dense, closely packed grains; Dr. Larmor as a rotationally elastic fluid. In Professor Mendeléeff's eyes the ether seems but a finer kind of matter. In those of his colleagues it is the one concrete reality; while matter, according to Professor Reynolds, consists merely of waves, and, according to Dr. Larmor, of systems of electrons, or nuclei of permanent etherial strains, in rapid motion.

How are we to reconcile these three diverse presentments of the ether? Perhaps the best answer I can offer is to remind my reader that throughout the history of science truth has ever been the offspring of diversity rather than of uniformity. Three men describing a neighbour's house might very well give discordant, and yet not untrue, accounts of it, accordingly as their own windows looked upon its front, its back, or one of its sides. It may be—I do not venture to say it is—that each of these seemingly diverse theories of the ether expresses something that is true about that aspect of the subject which chiefly has presented itself to its author.

MODERN THEORIES OF LIGHT

It must be confessed, I think, that the present aspect of some branches of physical science, as they may be supposed to present themselves to the untrained eye, is calculated to suggest the idea of a troubled sea rather than that of a quiet, cultivated plain. It must seem to most that in these departments the days of ordered, regular progress have passed away, and that, now, the times, though not less productive of facts than those that went before them, are stormy and exciting rather than truly illuminating.

And yet this is not really a true picture of the state of things. It only seems so if we choose our standpoint badly. Let me give an illustration. Imagine two people watching an express train—one from the platform of a station through which the train has just rushed, the other from a vantage point a little farther distant and a little to one side. What do they see? To the first, as the reader will discover if he tries the experiment, the train seems to move away fitfully; to take a succession of jumps, as it were. The second, on the other hand, forms a truer judgment; to him the movement of the flying train appears even and continuous; he sees none of the stops and starts that the man on the platform believes himself to have detected.

And so it is in regard to the progress of the branches of science that are in my mind. If we stand too near, or select our standpoint badly, if we concentrate our gaze on the last discovery, or even on the last few discoveries, much will escape our notice, and we shall form a picture very different from that seen by an observer who secures a wider field of view, so that he perceives whence each new departure starts, and can form an opinion as to what the direction of the next movement is likely to be. In short, if we wish to understand the significance of the discoveries in physics and chemistry that have startled the world during the last few years, we must stand back, as it were, so that we may see them in relation to the events which came before them. We must, in fact, know something of the steps which led up to them, something of the history of the group of facts or ideas to which they belong.

If Dr. Thomas Young could have revisited the famous lecture theatre of the Royal Institution in Albemarle Street, where once he taught, on the evening of Friday, April 19, 1901, when Professor J. J. Thomson was demonstrating the existence of particles a thousand times smaller than atoms of hydrogen, which move with velocities comparable with the velocity of light, and if he had been shown how these particles generate light when they fall upon dense obstacles, it seems likely he might have jumped to the conclusion that, after all, the wave theory of light, which he had done so much to establish, was dethroned, and that the corpuscular theory of Newton was about to reign once again in its stead. And yet one may hazard the guess that Professor Thomson was never more convinced than at that moment of the soundness of the wave theory.

The present position of the wave theory of light is not, I think, very difficult to understand if we get the facts before us in their proper relations; but to those who have not had an opportunity for doing this the whole question of the nature of light must have become very perplexing. They have understood for years that light consists of waves or ripples in something called "the ether"; but what is this they hear about the light-producing corpuscles in the radiations of radium, about a new theory which teaches that the Aurora Borealis is produced through the impacts of minute corpuscles shot out by the sun on the earth's atmosphere, and about the light-producing powers of the electrons in the cathode streams of the Crookes' vacuum tube? What have these to do with waves and the ether?

To get the answer to these questions, let us endeavour to trace the history of the wave theory; let us see how it stands at this moment, and what, if any, is the part played by these corpuscles in the generation of light waves.

Before we begin our survey, I must ask my readers to remember that light travels in straight lines with a velocity in air of about 186,000 miles per second. That when a ray of light passing through the air meets a solid object, such as a sheet of silver or glass, it may rebound, as it were, or be *reflected*, as happens when it falls on a polished silver plate, or may pass through the solid more or less completely, as happens when a light ray falls on a sheet of any transparent solid. That in this latter case the ray is turned out of its original course, *refracted*, at the surface of the solid, unless the direction of the ray is perpendicular to the surface of the solid, and that it is again diverted, only in the opposite sense, when, emerging from the denser medium, it passes again into the less dense medium, the air. Also, that white light is not homogeneous, but is a mixture, Newton having resolved it into its components, as we all know, by means of a triangular glass prism. And, finally, that the study of the spectrum by modifications of Newton's method has revealed the fact that all light is not visible. There being light, which we can detect photographically and in other ways, to which the human eye does not respond, some of these invisible rays being found beyond the visible part at the violet end of the spectrum, others beyond the visible part at the red end.

It will be found later that the fact that certain invisible radiations can be reflected, refracted, and polarised[1] like light is taken as evidence that though we cannot detect them with our eyes these radiations are of similar nature to light waves. On the other hand, the radiations of radium cannot be reflected, refracted, and polarised, and we have reason to think that in this case the radiations are largely made up of particles as large as atoms together with smaller corpuscles. We must not, however, draw the conclusion that all radiations which cannot be reflected, refracted, and polarised are corpuscular like those of radium. That would be going too far.

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[1] When a beam of light falls upon a plate of tourmaline cut parallel to the axis of the crystal only part of the light passes through the tourmaline, and the properties of the transmitted beam lead us to suppose that the vibrations are all executed in one plane, which is transverse to the direction of the beam; *e.g.* they might all be vertical vibrations, or all horizontal vibrations. Such light is said to be *polarised*.

In 1857 Whewell wrote of Optics that she had reached her grand generalisation "in a few years by sagacious and happy speculations." But Whewell was wrong if he supposed that the main structure of optical theory was then complete. For it was not so. Its foundations were laid, and well laid, by our fellow-countryman, Thomas Young, and Fresnel, the French engineer. But that is all. Faraday, Clerk Maxwell, Hertz, Zeeman and other master builders have been busy on the superstructure for half a century since then, and yet to-day physicists only begin to think they see what that superstructure is going to be.

The theory of light adopted by Newton was the "corpuscular" or "emission" theory. We all know that when a particle of flint strikes hard steel, flashes of light may be seen, and that not so very long ago fires were lighted and muskets discharged by sparks produced by means of these flashes. Such facts as these, perhaps, inspired the authors of the corpuscular theory. For this theory assumed that every self-luminous body emits very small material particles which cause the sensation of light when they enter the eye and fall upon the retina. To-day, the corpuscular hypothesis seems to us as "dead as a door nail," yet it had so much to recommend it that it has been said that if the existence of electrons and the modern experiments on the pressure of light had been known to Newton and his successors, it is possible we might not yet have heard of the undulatory theory; while those who are familiar with the story of radium will remember that we explain the power of this mysterious substance to illuminate the closed eye in the dark by an hypothesis which closely resembles the old corpuscular theory; except that in this modern instance it is not asserted that the retina responds directly to the corpuscles, but that it is chiefly excited by "light" given out by the cornea and lens of the eye, which become luminous under the influence of the β and γ rays of radium.

From the first formidable objections to the emission theory presented themselves. It accounted for much. For example, it was found to be consistent with the fact that light travels in straight lines from its source to the recipient, and it agreed with the observed laws of the reflection of light. But it broke down in other directions. Thus, according to the emission theory, light should travel more rapidly through such substances as glass, water, and diamond than through a gas such as air; but in reality light travels faster through the latter than through the former substances. Again, when a beam of light strikes a surface of glass, or some other similar transparent substance, some of it travels on through the glass and some is reflected from the surface, instead of all of it passing through the glass or all rebounding, as we should expect material corpuscles to do. This latter difficulty forced Newton so to modify the theory that it has been said to have acquired in his hands considerable resemblance to the rival hypothesis; because to account for the "fits of easy reflection and easy transmission," to which the corpuscles were supposed to be subject, Newton found it necessary to imagine all space to be filled with an "ether" in which the corpuscles generate waves on striking a reflecting or refracting surface, which waves, overtaking the corpuscles, assist or oppose their motions periodically, and thus determine whether at any fresh surface they rebound or pass onward according to the law of refraction and the nature of the substance. The late Professor Preston, in his treatise on light, sums up the objections to the emisson theory as follows: "We believe," he says, "an ingenious exponent of the emission theory, by suitably framing his fundamental postulates, might fairly meet all the objections that have been raised against it." But it will be found on an examination of the whole "that these necessary postulates endow the corpuscles with the periodic characteristics of a wave motion." But when this is done the corpuscles become superfluous, since wave motion alone has been shown to explain the phenomena sufficiently. It may be added here, though perhaps it does not greatly strengthen the argument for the wave theory, that now we have become aware of the existence in cathode streams and elsewhere of minute particles of the order of the corpuscles imagined by Newton which move with velocities comparable to the velocity of light we find that though these do, indeed, cause the sensation of light when they enter the eye, they do so not by direct impact on the retina, but indirectly by inducing fluorescence of the tissues in front of the retina; and that these particles, unlike true light, cannot be reflected, refracted, or polarised.

If it be admitted that light is a manifestation of energy, then, remembering that energy is by hypothesis indestructible, and that light from the sun takes about eight minutes to reach the earth, we have to find where this energy is stored during those eight minutes when it is neither on the sun nor on the earth, and by what it is carried across the space that separates us from the sun. The emission theory, according to which it is carried by minute corpuscles, we have already abandoned. But if the energy of light is not carried to us from the sun by these imaginary projectiles, the corpuscles, how then does it reach us? The only alternative that has been conceived, and it is found to fit the facts, has been to suppose that all space, and perhaps all matter, is full of a continuous medium, "the ether,"[1] and that the propagation of light depends upon waves in this ether, which when once started at any point in it by a luminous body, say by the sun, are propagated from one portion to another successively till they reach the eye or some other receiver. The idea of such a medium as is here suggested has seemed so little objectionable to philosophers in the past that at the time of Descartes ethers seem to have been invented on the slightest provocation, till at one time all space had been filled with them many times over. But to-day only one of them survives—viz. that invented by Huygens to account for the propagation of light.

Young, Fresnel, and the other workers who established the wave theory found themselves able to throw little light on the nature of the ether itself. If light consists of waves, then these waves must be waves in something or waves of something, and this something must be very different from anything we are at present acquainted with. It cannot be a solid like a stone, nor a liquid like water, nor can it be a gas, for the interstellar space and the most completely evacuated receivers transmit light, and, therefore, are filled with ether. It must be something vastly less dense than any gas, yet it must be incompressible and resist being torn or cut asunder more strongly than steel. It is not a jelly, for things can move about in it far more freely than in air, and yet it resists cutting and tearing, and propagates vibrations which are communicated to it. We assume its existence, and we deduce its properties from what we know about light and radiant heat ; and our conviction of the truth of these deductions has been strengthened by the discovery that these properties of the ether are those required to account for many electrical and magnetic phenomena. But what it is we do not know. When we expend energy in electrifying a body or in raising it from the earth's surface so that energy disappears, or becomes potential, as we say, it may be that the energy passes somehow into the ether, much as the energy of light is stored in the ether during the period which elapses between its disappearance from the sun and its reappearance at or near the earth's surface eight minutes afterwards.

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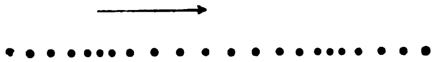
[1] It might be more correct to think of the particles of matter as bathed in the ether.

The nearest approach to an idea of the properties of the ether can be got, perhaps, by making a simple experiment with a large lump of jelly and two hat pins. If you stick the hat pins into opposite sides of the jelly, and make one of them quiver up and down, or from side to side, or in a circular direction, you will find its motions are so conveyed to the second hat pin that this also moves in a corresponding manner. The waves which start from the first pin find their way through the jelly to the second without undergoing any change in their general character. Note, however, that this does not show that the ether is a jelly. It only affords us an ocular demonstration of the transference of energy from one point in a medium to another by waves of a particular kind.

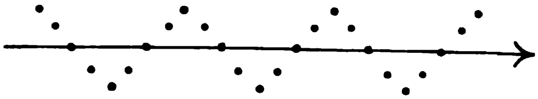
We must admit that the ether permeates not only the interstellar space that separates the heavenly bodies, but also all those forms of matter by which luminous, electric, and magnetic disturbances can be transmitted. For the vibrations we meet with among the material particles of such substances as glass or air travel far too sluggishly to allow us to suppose that light, for example, may be conveyed by vibrations among their molecules, though we may believe these molecules are not without their influence, when we remember that light does not travel at all through some things and travels with different velocities through those which do transmit it.

If we throw a stone into a pool of still water, we see a succession of waves start from the point where the stone strikes the water and spread out to the banks in every direction. And those who have studied the phenomena of sound tell us that this is propagated by waves in the air which spread from the source of the disturbance in every direction in an analogous though not in an exactly similar manner. But when we seek to explain the propagation of light by waves, two difficulties arrest us. First, as we have already seen, since light is propagated in the most perfect vacuum, and through the interstellar spaces as readily as in glass, water, or air, we must discover or invent something in which its waves can be generated, and through which they can move in space. This we have accomplished by means of the ether. Secondly, that waves like sound waves will not, it is found, account for the actual characteristics of light in a satisfactory way.

If we think of wave motion in a gas or liquid, and conceive it to depend on movements of the molecules of the substance concerned, we shall see that these movements might, in effect, take place in several ways. Imagine, for example, that waves pass in the direction of the lines of type on this page, and from left to right, and that the printed letters represent the particles of matter. Then you will see that the waves might be produced by the particles oscillating longitudinally, backwards and forwards, along the line of propagation in such a way as to produce alternate states of compression and rarefaction as suggested below:—



And again, on the other hand, that the particles might swing across the line transversely, upwards and downwards, so as to produce undulations of quite a different order, like those suggested by this second figure :—



Now sound is propagated through the air by waves of the first kind—by longitudinal waves, and in the early days of the undulatory theory luminous undulations were supposed to be of a similar character. Unfortunately, however, the phenomena of polarisation could not be accounted for by waves of this kind; and this led Newton to reject the wave theory, and delayed its final triumph till Fresnel, following up a suggestion made before his time that light waves might be transverse waves, overcame the difficulty, and so cleared the way for a new era in optics.

The wave theory of light had hardly become familiar when Faraday, in 1845, made a discovery which, though it stood alone for a time, was the herald of some of the greatest triumphs of science in modern times. This discovery revealed the pregnant fact that a strong magnet exercises a peculiar action on polarised light, and established a relation between light and magnetism which gave a new turn to the trend of thought, and resulted, in the end, in nothing less than the annexation of the science of light by the science of electricity.

If you turn over the pages of some modern book on electricity, very likely you will discover that to-day the electrician no longer thinks all electrical phenomena can be explained sufficiently by the hypothesis that electrified bodies carry something, called electricity, which can flow through the pores, or perhaps even through the substance, of many forms of matter, more or less as water flows through a tube. Your author may find it convenient to discuss many phenomena from the point of view of this hypothesis, but you will discover that sometimes he is thinking of wave motion rather than of the flow of any kind of fluid.

It has now long been known to us that masses of matter may be set in motion by making use of the repulsions between magnets and circuits carrying currents of electricity. Again, modern research shows us that electrical disturbances like those made use of in wireless telegraphy can be propagated, like light, at vast velocities through great distances, even though there be no tangible connection between the source of the disturbance and the distant receiver by which we can suppose the disturbance to be carried. Facts like these, when carefully considered, could not fail as they accumulated to change the aspect of electrical theory. For if we admit that energy is indestructible, then, when it disappears from one spot and subsequently reappears at another, it must have continued in existence, as has been said before, in the interval. This consideration seems to compel us to conclude that in the case of electrical disturbances such as those we are thinking of, as in the case of light, the energy concerned must be conveyed by the aid of a medium of some sort or other — possibly, indeed, by the ether.

But does not this suggest an analogy, more or less close, between the electrical disturbances in question and light itself? And if there be such an analogy; if there be waves of electricity as well as waves of light, then, is it not clear that we have a definite and most interesting problem presented to us, viz. to find whether electric waves and light waves are similar or dissimilar in their properties, and, in the latter case, to ascertain what, if any, are the relations between them?

The first "electromagnetic theory of light" was proposed by Faraday "as a matter for speculation" in 1846. But the first real step towards the establishment of such a theory was taken by Clerk Maxwell some years later, when he found the "elasticity of the magnetic medium in air to be the same as that of the luminous medium, 'the ether,' if," as he says, "these two co-existent, co-extensive, and equally elastic media are not rather one medium."

Clerk Maxwell's work is not easy to follow, and it could not be made clear within the limits of a short essay. But much may be learnt about it from a perusal of Professor Glazebrook's "Life of Maxwell," or from that fascinating book, "Modern Views of Electricity," by Sir Oliver Lodge.

Put briefly, the theory is this: That light consists of electric vibrations in the ether, and that light waves are electric waves. Light waves and electric disturbances travel through space and through the air and some other substances with the same velocity, but they differ in this respect, that while many thousands of the longest visible light waves can be packed within an inch, the electrical disturbances are of far greater length. An electric wave may be feet, miles, or even thousands of miles in length, and none, I think, have been detected of much less length than two or three inches. All our methods of exciting the small light waves by burning oil or gas, by igniting solids like the materials of the incandescent mantle, or by means of electricity, are very wasteful. A boy turning a handle, as Sir Oliver Lodge has pointed out, might replace the boilers and dynamos of an electric light installation if we knew how to excite and maintain electric vibrations of sufficiently small wave length. How to do this is one of the practical problems which the electrical engineer has to face.

Maxwell's theory was first proposed about 1861. By 1888 it was generally admitted in this country that an electrical disturbance at any point is transmitted from that point by means of the ether, and that electric vibrations like waves of light, and unlike sound waves, are at right angles to the direction in which the waves are propagated—that is to say, they are not different in kind from light waves. But these ideas were by no means generally accepted in other countries until Hertz, about the latter date, set himself the task of testing them experimentally.

A theory, above all, we must remember, is a tool—a tool with which we may win knowledge. But the idea suggested by a theory must be verified. If invisible electric disturbances truly be light waves, then should it not be possible to reflect, refract, and polarise them?

This problem was attacked by Hertz at the suggestion, I believe, of Helmholtz. And with complete success. He invented means of recognising the invisible electric waves and of measuring their wave lengths. He found that they travel in air with the velocity of light. He showed that they could be reflected, refracted, and polarised—that is to say, he showed they had the characteristic qualities of light.

Now let us turn our attention for a moment to the electrons or corpuscles which Dr. J. J. Thomson recognised some years ago in the cathode streams—those electrified particles, nearly a thousand times smaller than atoms of hydrogen, which generate light when they fall upon responsive matter, which can illuminate the eye, and which are thought to be the cause of the Aurora Borealis. Is it not possible that these may be the corpuscles of Newton? That there is, as it were, light of two kinds—corpuscular light, and light due to wave motion? This question, I think, may very well have arisen in the minds of those who are interested in these matters, but have been unable to follow them closely. On the whole the answer must be "No." The electrons or corpuscles emitted by radium, and found in the cathode streams, no doubt are of such an order of minuteness as those conceived by Newton. They move with velocities which, though usually less than the velocity of light, may be compared with the latter. They render the front of the eyeball and many other things luminous when they fall upon them. And it is suggested that they are shot off from the sun, and then, falling upon our atmosphere, are the cause of the beautiful Northern Lights; that is to say, they can carry energy across the interstellar space, and yield it up again as light at the end of their journey. But they want the essential characters of light. They have not been reflected, refracted, and polarised.

And yet, according to one view, the electrons, though they are not light, play an important part—perhaps, I should say, an all-important part—in the generation of light. For it is suggested by advanced workers in this field that the electrons or corpuscles may be regarded as a sort of electric appendage of the chemical atoms capable of moving in or round the atoms; in fact, that they may be a sort of electrical satellite of the atoms. And, further, that it may be the pulsations of the electrons which give matter its connection with the ether. That it may be the vibrations of the electrons of the atoms during accelerative periods which give the latter their power of generating waves in the ether; in which case the kind of vibration produced by any atom would depend upon the vibrations of the electrons it carries, every frequency of vibration corresponding to a definite line in the spectrum.

But electrons carry negative charges of electricity; therefore we should expect an electron revolving round or in an atom to behave like an electric current. And what we know about the influence of magnetic fields on electric currents would lead us to expect that the motions of electrons could be altered by means of magnets. But changes in the motions of the electrons of atoms would alter the character of the waves generated by them in the ether—that is to say, it would modify the refrangibility of the light produced. Now this is a change that we might hope to detect by means of the spectroscope, for it might be expected to reveal itself in the case of any given element by a multiplication of the lines in the spectrum of that element; a given line being doubled, for example, or tripled, or even altered to a further extent. The possibility that magnets might produce effects of this kind on light was perceived, on theoretical grounds, so long ago as 1895 by Dr. Larmor, of Cambridge, and such effects had, in fact, been looked for much earlier by Faraday, though he did not expect to find them on the above grounds. In 1897 they were actually observed by Zeeman in the case of the spectrum of sodium, and since then a number of workers have verified and added to his observations, thus bringing fresh support to the electromagnetic theory, strengthening our belief in the existence of the electrons, and supporting this "electron theory of radiation," which we owe to the work and thought of Dr. Johnstone Stoney, Dr. H. A. Lorentz, Dr. Zeeman, and others.

And now, in conclusion, let us return for a moment to the point from whence we started. Observe how the combination of hypothesis and experiment has proceeded regularly and gradually, step by step. First, the wave theory of Young and Fresnel ; then the electromagnetic theory of Clerk Maxwell, and its verification by Hertz ; then, again, the discovery of electrons, and the idea that these may confer upon atoms the power of generating wave motions in the ether ; while the "Zeeman effect," only observed in 1897, falls into its place, and helps to bind the whole together. Here, whatever may be the fate of "the electron theory of the magnetisation of light," is nothing but orderly progress.

WEIGHING A WORLD

SCIENCE, "the great measurer," is for ever busy with scales, weights, and measuring-tape. Directly it was settled that the world is round, we find the Alexandrian astronomers attempting to measure its circumference. Hardly had Newton formed his theory of gravitation before his mind was full of schemes for "weighing the earth." From the moment when the modern atomic hypothesis was accepted, and indeed even before, Dalton and his colleagues were as busy as bees trying to weigh invisible, nay, hypothetical, atoms and molecules. And the discovery of the "electrons" or "corpuscles" in Sir William Crookes' vacuum tubes may almost be said to have consisted in attempts to compare their masses with those of the lightest particles previously known—atoms of hydrogen. Nothing seems too difficult. The weight of the earth, the weight of an atom, the velocity of light, nay, the speed of thought itself, or, at least, the speed with which thought can be translated into action, all these and a thousand other quantities have been brought by science within the compass of her measuring instruments, their values ascertained, stated in familiar terms, and placed, gratis, at the service of man.

Perhaps my readers may feel disposed to take a peep into the machinery employed to accomplish the tremendous task of weighing a world? If so, I must ask them, first, to consider this question:

"What do we mean by "the weight of the earth"?

When we speak of the weight of such an object as a lump of coal, we mean, of course, the pull of the earth upon that piece of coal; and the quantity of coal we call a pound is that quantity which is pulled to the earth with a force just equal to the force that pulls a particular piece of platinum, marked "P.S. 1844 1 lb.," and called the "Imperial Avoirdupois Pound," which is kept at the Standards Office in Westminster.

Now it is clear that the earth, as a whole, cannot pull itself to itself. Every particle of it in every direction must pull every other particle, with the result that there is a state of equilibrium and no pull; and thus, in the everyday sense of the term, the earth has no weight at all.

But we all know that though when we weigh bodies we may seem merely to measure the pull of the earth upon them, we not only learn the strength of this pull, but also measure what Newton called "the quantity of matter in them," or, as we say to-day, "their masses." For it has been shown by Newton that at any given point on its surface the earth's pull on an object is proportional to the mass of the object, and independent of such qualities or considerations as its shape or position, whether it is a solid, a liquid, or a gas, and also, as Lavoisier has taught us, independent of its chemical constitution. This being, of course, only a particular case of Newton's law of gravitation, which tells us that every particle of matter in the universe attracts every other particle with a force which depends on their masses and on the distances which separate them; the attraction being proportionately greater between large masses than between small masses, increasing when the masses approach one another, and decreasing as they recede, in such a manner that if the distances between the centres of two spheres be doubled, then the attraction between them is reduced to one-quarter of its original strength.

Returning now to our question, we see that the process familiarly termed "weighing the earth" consists really in measuring the quantity of matter the earth is made of, or, in modern terms, in determining its mass.

Although we cannot even imagine ourselves balancing the earth on a pair of scales against a set of weights, some other way of attacking the problem which is not altogether beyond the range of the imagination may occur to the reader, and help him to grasp its nature and difficulty.

We know, for example, that the diameter of the earth is about 8000 miles, and we know how to calculate the approximate value of a sphere when we have measured its diameter. Why, then, should we not calculate the volume of the earth in cubic feet, find the mass of a cubic foot of it in pounds by weighing samples, finally multiply these two quantities, and so determine its mass in pounds? It would not be very difficult to perform these simple operations, but, unfortunately, even if we neglect the irregularity of the earth's surface, there are still some fatal objections. The masses of equal volumes of rock taken from different parts of the earth's crust vary considerably ; and, further, even if this were not so, we have no means of getting samples of the material of which the earth is made except by scratching its outer skin, and it would by no means be safe to assume that the average weight of each cubic foot of the rocks which exist below, out of our reach, is the same as the average weight of each cubic foot of the rocks which are familiar to us on its surface. Still, the general idea of the problem presented in the form of this faulty proposal is not unhelpful. It simplifies the matter considerably. We know the volume of the earth more or less closely, therefore all we have to do is to find its "mean density"—to find, that is, what proportion the mass of the earth bears to the mass of a globe of water of equal size. When this is done, since every cubic foot of water weighs about 62½ lbs., we can easily calculate the weight of the earth in the ordinary sense of the term, and state it in pounds or tons, in grams or kilograms, as we may desire.

The process of "weighing the earth," then, may be said to consist in finding its mean density, water, which is said to have the density 1, being taken as the standard substance. Thus stated, the problem seems easy enough, but the solution of this simple problem has occupied the thoughts of many master minds, and taxed to their utmost the powers of many great experimenters from the days of Newton.

It is true that, by taking the earth as their standard, astronomers have been able to draw up a table of densities for the heavenly bodies, from which we learn that the mean density of the sun is about one-fourth as great as that of our globe, that of Venus about nine-tenths as great, that of Mercury one and a quarter times greater, and so on. But this, though sufficient for many purposes, fails to give us such a clear idea of the matter as we get when we can think of our quantities in familiar terrestrial standards such as the gram or the pound; and so it is necessary to connect the celestial scale of densities, in which the earth is made the standard, with one of the more familiar terrestrial scales. The first attempt to do this was made by Newton. This attempt was a mere estimate—in fact, a guess. I give it in full in his own words, as translated by Motte :—

> But that our globe of earth is of greater density than it would be if the whole consisted of water only, I thus make out. If the whole consisted of water only, whatever was of less density than water, because of its less specific gravity, would emerge and float above. And upon this account, if a globe of terrestrial matter, covered on all sides with water, was less dense than water, it would emerge somewhere; and, the subsiding water falling back, would be gathered to the opposite side. And such is the condition of our earth, which, in a great measure, is covered with seas. The earth, if it was not for its greater density, would emerge from the seas, and, according to its degree of levity, would be raised more or less above their surface, the water and the seas flowing backwards to the opposite side. By the same argument, the spots of the sun which float upon the lucid matter thereof are lighter than that matter; and, however the planets have been formed while they were yet in fluid masses, all the heavier matter subsided to the centre. Since, therefore, the common matter of our earth on the surface thereof is about twice as heavy as water, and, a little lower, in mines, is found about three or four or even five times more heavy, it is probable that the quantity of the whole matter of the earth may be five or six times greater than if it consisted all of water, especially since I have before showed that the earth is about four times more dense than Jupiter.

Newton's guess, curiously enough, hits the limits between which the values subsequently fixed by experiments are mostly to be found.

In practice, all the methods of weighing the earth resolve themselves into experiments in which we measure the attraction between two bodies having known masses placed at a known distance from each other on the earth's surface, and then compare this with the attraction of the earth on some known mass of matter, also on its surface. The following illustration, taken from a lecture by Professor J. H. Poynting, will make the idea clearer :—

Suppose you hang a weight of 50 lbs. from a spring balance a few feet above the earth. Then the pull of the earth, whose centre is about 4000 miles or 20,000,000 feet away, is 50 lbs. Now suppose you bring a second weight, this time, let us say, a weight of 350 lbs., to a position one foot from the first one, and between the latter and the earth, so that its pull is added to that of the earth. Then, if your balance is sufficiently sensitive, you will find the smaller mass no longer weighs 50 lbs., but a little more—in fact, about $\frac{1}{250}$ of a grain more—that is to say, the pull of the 350-lb. weight at the distance of a foot is equal to the $\frac{1}{250}$ of a grain, or $\frac{1}{1750000}$ of 1 lb., or the pull of the earth at a distance of 20,000,000 feet is about ninety million times as great as that of a sphere of 350 lbs. at one foot, for

$$1{,}750{,}000 \times 50 = 87{,}500{,}000.$$

If the earth could be placed at an average distance of one foot from the 50-lb. weight, instead of at a distance of 20,000,000 feet, its pull would be proportionately greater—viz. about four hundred billion times greater, so that at equal distances the pull of the earth would be four hundred billion times ninety million times that of a 350-lb. sphere. But, as already explained, at equal distances these pulls are proportional to the masses concerned, and thus, by doing a little more arithmetic, we should find that the earth weighs about 12,500,000,000,000,000,000,000,000 lbs. Finally, if we calculate the mean density of the earth from these figures and from its volume, which can be deduced from its diameter, we find that its mass is about five and a half times as great as that of an equal volume of water, or, to use the technical term, that the "mean density" of the earth is five and a half times as great as that of water. This, however, is only the result of an imaginary experiment. The real thing, though similar in principle, is far more complicated, as will easily be understood when I mention that a determination of the density of the earth carried out with due precautions to eliminate all sources of error may occupy several years, and that in some cases the necessary operations are of so delicate a character that the mere passage of railway trains in the neighbourhood of the apparatus may be a serious source of trouble. Indeed, on one occasion

Professor Boys, when working at Oxford, was stopped by an earthquake which occurred thousands of miles away, and was, I believe, only detected in this part of the world through the circumstance that Professor Boys was weighing the earth when the wave reached these regions.

The actual objects whose attractions have been studied in attempts to weigh the earth have varied very widely. The earliest observers studied the attractions of mountains on objects brought near them ; Professor Boys those of small metallic spheres, the largest of which were only four and a half inches, and the smallest one-fourth of an inch in diameter. The methods employed divide themselves into three or four groups.

First come experiments in which the attraction of a mountain or some natural object, such as a zone of known thickness of the upper crust of the earth, is compared with that of the earth as a whole.

Secondly, the famous " Cavendish experiment," in which the attractions between metallic masses quite small in size are investigated by means of what is known as a torsion balance.

And, thirdly, researches in which common but very delicate scales and weights are employed. Some very beautiful experiments falling within this last class were made a few years ago at what was then the Mason College, Birmingham, by Professor Poynting, on whose publications on the subject of the weight of the earth this essay is largely based.

And now, after all these preliminary remarks to clear the way, we come to the real thing, to the actual experiments made for the purpose of weighing the earth, from the time of Newton, who inspired all this work in which our fellow-countrymen have always played a conspicuous and successful part, till to-day.

We have learnt from the preceding pages that astronomers have succeeded in comparing the densities of various heavenly bodies by means of astronomical observations, and have drawn up tables stating their results in terms of the density of the earth, but that if we wish to get out our results in earthly measures, such as ounces or grams, we must descend from the stars, and compare, for example, the pull of the earth on some object on its surface with the pull of some measurable mass on the same object. All this, of course, was very well understood by Newton, who saw, further, that the power of a mountain to deflect a plumb-line might be employed; unfortunately, he concluded that the effect would be too small to measure, which, indeed, may possibly have been true at that time. Newton also investigated the possibility of measuring the attraction between large spheres, and calculated how long it would take a sphere one foot in diameter, and of equal density with the earth, to draw a second sphere, of the same dimensions and equal density, placed a quarter of an inch away, across this interval of a quarter of an inch. Through a mistake in his arithmetic, he found the required time to be about a month, which is vastly more than the few minutes that would really be needed, and as such a rate of motion was utterly beyond measurement, he confined himself to making the celebrated guess mentioned above. But not very long afterwards both these methods were put to the test of experiment with a considerable degree of success.

Some doubt is said to exist as to whether Newton was the real author of this mistake, but, as Professor Poynting remarked in a lecture at the Royal Institution a few years ago, there is something not altogether unpleasing in the belief that even Newton could make a mistake. His faulty arithmetic showed that there was, at any rate, one quality which he shared with his fallible fellow-men.

When the attractive force of a mountain is to be studied, the experiment, in its simplest form, is somewhat as follows: A weight hanging at the end of a thread—that is, a plumb-line more or less similar to the plumb-line employed by a mason, but far more sensitive and provided with more exact means of measurement—is placed first in some suitable position not too far away from the mountain, but beyond the range of its sensible attraction, and its position noted on a scale of divisions when it hangs freely suspended, and, therefore, perpendicular to the earth's surface. The plumb-line is then brought up as close as may be to one side of the mountain. When this is done the plumb-line is found to be drawn a little to one side of its previous line of suspension. That is to say, a little out of the perpendicular and towards the mountain. The amount of this displacement is measured on the scale of divisions, and the length of the plumb-line is also measured. From these data the astronomer can calculate the ratio of the horizontal pull of the mountain to the pull of the earth.

Finally, the mountain is carefully surveyed and the densities of pieces of the rock of which it is composed are measured. Knowing these densities and the volume of the mountain we can estimate the mass of the mountain in pounds or kilograms, according to the system selected; and when this is done we know the mass of the mountain, the pull of the mountain, the pull of the earth, and their distances, and from these, knowing the law of gravitation quoted above, we can deduce the other quantity involved, the mass of the earth.

The first investigator to actually determine the mean density of the earth by this method was M. Bouguer, who was a member of one of two scientific commissions sent out from France about 1740 to measure the lengths of degrees of latitude in Peru and Lapland—that is, at points near to and remote from the equator—in order to settle finally the shape of the earth, whether it is flattened at the poles, as Newton supposed, or drawn out, as had then lately been suggested. The members of these commissions, which, by the way, settled the question in favour of Newton's views, did not confine themselves to investigating the shape of the earth; and M. Bouguer, in particular, seized the opportunity of testing the "mountain mass method" of weighing the earth thus afforded him by his visit to the great mountains of the Andes. M. Bouguer made two distinct sets of measurements. In the first he studied the swing of a pendulum at the sea-level, then at a point 10,000 feet higher, on the great plateau on which Quito stands, and, finally, on the top of Pichincha, which is about 6000 feet above Quito. He knew that if a pendulum were lifted to a great height above a wide plain or over the open sea, say, for example, by means of a balloon, its swing would gradually grow slower as gravity diminished at the higher levels; and he calculated from the swing of his pendulum at Quito that gravity at that place was greater than the calculated amount for the height at which he worked, owing to the downward pull of the great tableland beneath him.

Bouguer's second set of observations was made near Chimborazo, a mountain 20,000 feet high, by the plumb-line method as described in outline above, only in a far more refined form. His difficulties were very great, for he was obliged to work above the line of perpetual snow. His labours began with a troublesome and even perilous journey of many hours over rocks and snow-fields, and when the site selected for the first set of observations was reached he had to fight against snowfalls, which threatened to bury the instruments, the tents, and even the observers themselves. At the second station, which was below the snow-line, he hoped for better conditions ; but here he encountered gales of wind, and it was still so cold as to hinder the working of his instruments. Under these circumstances it is not surprising that the results obtained were, as Bouguer himself recognised, of little permanent scientific value. The cause for wonder was that he got any results at all. But his time and labours were not wasted. His observations proved that the earth, as a whole, is denser than the mountains upon it ; that it is not a mere hollow shell, as some people in those days still supposed, nor yet a hollow globe filled with water, as others had insisted. Besides, he had broken new ground, and before very long his experiments were repeated under more favourable conditions and with better results.

The next experiment by the mountain-mass method was made in the neighbourhood of Schiehallion, in Perthshire, thirty years later, under the auspices of the Royal Society, who, at the instance of Maskelyne, then Astronomer Royal, appointed "a committee to consider of a proper hill whereon to try the experiment, and to prepare everything necessary for carrying the design into execution."

A few years ago the inhabitants of a certain remote island were considerably excited by the absurd proceedings of a party of visitors to their shores, who did many things which seemed stupid, not to use a stronger term, to the islanders, and at length lost the last vestiges of their respect by boiling water in tin pots on a mountain top in order to find out how high the mountain was. I have sometimes wondered what the hard-headed natives of Perthshire thought of the party of gentlemen who came to Schiehallion about the year 1774, and proceeded to watch plumb-lines hanging in the air, and to peep at stars through telescopes in order to discover the weight of the earth. But, be that as it may, after two months or so spent in observing, and two years more in surveying the mountain, making contour maps giving the volume and distance of every part of it from the two stations at which the observations of its attraction had been made—for Maskelyne did not follow the method of Bouguer exactly, but observed the attraction of the mountain from two opposite sides—and after determining the density of various fragments of the rock of which Schiehallion is composed, Maskelyne and his colleagues came to the conclusion that the mean density of the earth must be four and a half times that of water—that is, that the earth must contain four and a half times as much matter as a globe of water of its own size, or, again, that its mass must be equal to that of a globe of water four and a half times as big as the earth. This value was presently raised to five, as the result of further determinations of the density of the rock, and we have every reason to suppose that this latter value is not very far from the truth.

I should tire my reader were I to go further into this part of our subject and describe one by one the various inquiries following more or less similar lines that have been made since the completion of Maskelyne's celebrated experiments. Moreover, interesting and ingenious as these inquiries were, all were vitiated by a fatal defect. The accuracy of the conclusions reached depends in every case on two chief points. First, correct measurements of the attractive forces of the mountain masses studied are necessary, and this, doubtless, was attained in many if not in every one of the various investigations. Secondly, a fairly correct knowledge of the density of the rocks forming the mountains is required, and here the experiments in every case break down. We cannot learn with certainty the mean densities of the rocks forming a mountain ; at the best we can only make rough guesses at them. Consequently, of late years the attention of astronomers has been turned to the other methods to which I have alluded. These, though equally difficult to carry out, are not subject to this fatal objection. I may point out, however, before we proceed, that it would be quite reasonable, now the weight of the earth has been fixed by these other and sounder methods, to turn the above experiments about and apply the results obtained to the complementary problem of "weighing mountains."

"Of all experiments," exclaimed Professor Boys, a few years ago, in the course of a lecture at the Royal Institution, "the one which has most excited my admiration is the famous experiment of Cavendish." For this method of weighing the earth no costly expeditions to distant mountains, and no elaborate surveys requiring years for their performance are demanded. For the "Cavendish experiment," in fact, nothing is wanted but a few bits of wire, some strips of wood, balls of metal, and a case to protect the apparatus from "the wind," as Cavendish expressed it. If you possess these and certain other similar trifles, and if you possess, also, the genius for experimenting of a Cavendish or of a Boys, you can weigh the earth. If, in addition, you possess one of the wonderful silica threads discovered a few years ago by Professor Boys, you can construct an apparatus hardly too big to go inside a man's hat-box, with which you may do the thing to a nicety.

That great though most eccentric man, the Honourable Henry Cavendish, was, as I have said, the first to carry out in a laboratory the operation of weighing the earth, but the actual originator of the Cavendish experiment was the Rev. John Michell, who constructed the necessary apparatus, but died before he had an opportunity of testing the value of his ideas by making an experiment. After Mr. Michell's death his apparatus passed into the hands of Dr. Wollaston, and he handed it on to Mr. Cavendish, who, after making some modifications, performed the first "Cavendish experiment" in 1797–98. Cavendish found the mean density of the earth to be 5.45 times that of water, and we may take it that this was the first really trustworthy measurement. The experiment, in outline, was as follows :—

Two equal balls of lead, each two inches in diameter, were attached to the remote ends of a light wooden rod six feet long. This rod was suspended horizontally at its centre, by means of a wire forty inches long, inside a narrow wooden case to protect it from draughts. Outside the case two much more massive balls, also of lead, twelve inches in diameter, were suspended by rods from a beam, which worked on a pivot. This pivot was placed above the wire by which the rod carrying the small balls was suspended, so that the large balls could be swung at will into various positions outside the case. For example, they could be placed transversely by putting the two beams at right angles to one another, or brought close up to the smaller balls, one large ball to each small ball, on opposite sides of the case. The movements of the ends of the light rod within the case were measured by means of divided scales provided for the purpose, which were viewed from a distance through telescopes. In making an experiment the two large balls were brought up close to the two small balls, one large ball to each small ball, on opposite sides, so that the latter were pulled in opposite directions. This set the ends of the light beam swinging about a centre which could be determined by observing the range of successive swings by means of the divided scales. The large balls were then carried round to the opposite sides of the case, and brought close up to the small ones as before. The result was, of course, that the directions of the pulls upon the latter were reversed. The centre of the swing was again determined, and it was found not to be the same as at first. Many corrections had to be introduced, and so the working out of the results was not very simple, but they show that the earth has a mean density of 5.45. The Cavendish experiment has often been repeated, and Baily (a London stockbroker by profession) performed no fewer than two thousand one hundred and fifty-three of these delicate experiments in his laboratory at Tavistock Place between the years 1738 and 1742, obtaining the value of 5.66.

The Cavendish experiment, as I have said, has often been repeated, with various improvements, but never in a very much more perfect form till a few years ago; and in the interval Professor Poynting and others have succeeded in weighing the earth by means of common scales and weights. The experiment, in Professor Poynting's hands, consisted in hanging two 50-lb. weights on the opposite sides of a large, strong balance placed inside a suitable case; measuring the effect of bringing a large mass of metal, 350 lbs., under one of the 50-lb. weights, which increases the pull upon it to a measurable extent, and then transferring the large weight to the other side of the balance so that its pull upon the other 50-lb. weight could also be measured. The changes to be observed, of course, were extremely small, mere fractions of a milligram, in fact, and all sorts of precautions had to be taken to avoid the disturbing effects of draughts and other causes of error. The balance was placed in a cellar, and observed by means of a telescope through a small hole in the ceiling from the room above it. So delicate was the apparatus that if any one walked about the house when Professor Poynting was at work he was unable to make an observation, on account of the quivering of a mirror attached to the balance to enable him to observe the reflection of a scale through the telescope; and when this difficulty was overcome by placing the instrument on great blocks of indiarubber, and the balance had worked well for a whole year, it began to go wrong one day owing to the floor of the cellar tilting whenever he moved the large weight from one side of the balance to the other. The tilt was so slight that had the floor been ten miles long, one end of it would have been raised only one inch higher than the other end ten miles away, and yet this minute disturbance very seriously affected his observations. These are only a few of the difficulties encountered, but gradually they were overcome, and the density of the earth was found to be 5.493. Professor Poynting indicates the minute effect produced by the movements of the 350-lb. weight by the following apt illustration:—

Suppose all the inhabitants of the British Isles, say 40,000,000 persons, were placed in one pan of a gigantic pair of scales, and that they were counterpoised by weights, do you think the addition of one middle-sized boy to the population of the scale pan would be observed by the man who was weighing them? That is the sort of difference that had to be measured—a difference of one part in seventy or eighty million parts.

It will give a still better idea of the degree of perfection to which the art of weighing was brought by Professor Poynting if I add that the degree of accuracy attained was such as would be required to detect, in this imaginary experiment, whether the boy had both his boots on.

But splendid as this work was, the high-water mark was reached, perhaps, by Professor Boys in a recent repetition of the Cavendish experiment. Cavendish, as I have said, suspended the beam of his "torsion balance" by means of a fine wire, and the accuracy of his results depended on the elasticity of this wire. Now, unfortunately, metallic wires are not perfectly elastic, and when frequently used are subject to "fatigue"; and so there was a defect in the experiment, which remained uncorrected until a few years ago, when Professor Boys discovered how to produce threads not subject to this defect. These astonishing threads, which are made of melted quartz, are finer by far than the finest wire—so fine, in fact, that a single grain of sand spun into one of them might yield a thread a thousand miles long; moreover, they surpass steel in strength, and are marvellously elastic. Armed with quartz threads Mr. Boys was able to reduce the size of the Cavendish apparatus, and at the same time greatly to increase its sensibility. This and great personal skill enabled him to make what is probably the best measurement yet obtained of the earth's mean density—viz. 5.5270.

And so we find that the work of Maskelyne, the work of Cavendish, the work of Poynting, that of Boys, and, indeed, that of half a score others about whom I have said nothing, supports, almost without an exception, Newton's guess at the weight of the earth.

ON WEIGHING ATOMS

TO those who cull their knowledge of current science partly, at least, by means of occasional glances at more or less distorted images of single facets, such as are to be seen from time to time in the columns of the daily papers, I fear the title of this essay may suggest that it is somewhat belated. Atoms! I hear them say, what is he thinking about? There are no atoms now, they have all been cut up into electrons and corpuscles. Who cares about the weights of the atoms at the beginning of the twentieth century?

And yet never, perhaps, since Dalton propounded his atomic hypothesis a century or so ago has the existence of these hypothetical particles seemed quite so probable, quite so believable as to-day. True it is that within the last few years some of our ideas about the chemical atoms have been somewhat shaken. The hydrogen atom is no longer considered the smallest particle. If radium be indeed an element—and no one can deny that it exercises some of the functions of an element[1] —then the atoms of Dalton can no longer be regarded as indestructible individuals, but rather must be looked upon as congeries of still smaller bodies, each atom forming a kind of diminutive heavenly system, such as we might picture to ourselves by thinking about what we should see, or of what we should not see, if we gazed upon the heavens through the wrong end of an immense and powerful telescope. Yet, after all, the idea of the chemical atom remains, and the part it plays is not less but even more important than of yore. Still, the basis of most chemical speculation, the hydrogen atom, now, in addition, affords the physicist a jumping-place, whence he starts on some of his amazing flights into the regions where matter, energy, and electricity dissolving, as it were, into one another, almost escape the scrutiny even of his penetrating glances.

Here, then, is my excuse—and you have only to read Professor J. J. Thomson's lecture on "Bodies smaller than Atoms" to see that it is a good excuse—for asking my readers to hark back, and dwell for an hour on such an old-time subject as the methods of weighing the chemical atoms.

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[1] See a subsequent essay.

In the preceding essay I endeavoured to give those who are interested in matters of this kind a peep into the processes by which science has succeeded in weighing the earth. The great difficulty, or rather one of the great difficulties, in weighing the earth is its bigness. We not only cannot by any means get the earth into a scale pan, but we cannot even form a mental picture of such a process. When we contemplate the task of weighing an atom our difficulties are of the same order, but of the opposite kind. For atoms, if they exist, are far too small to be isolated. Think how many chemical atoms go to make up a single cubic centimètre of water, that is about as much as would go inside the shell of a small filbert, say, about 90,000,000,000,000,000,000,000 (ninety thousand million billion), and you will realise the character of the problem which John Dalton, of Manchester, presented to science when, by formulating his Atomic Theory, he made it an object to determine the sizes and masses of the atoms of the elements. How were Davy, Wollaston, and their colleagues, expert experimenters though they were, to perform a feat like this with the means then at their disposal?

How were they to weigh particles that could not be seen by means of the most powerful microscopes, nay, to be exact, particles which very possibly might exist only in the minds of Dalton and his followers? Let us see how this task has been accomplished.

From the earliest times philosophers have pondered on the constitution of matter. Does everything consist of grains held together by some attracting force, or is matter continuous, homogeneous, much as a jelly seems to be to the human eye? That is the question. The poet-philosopher Lucretius and others among the ancients, and in more recent days the great Newton ranged themselves on the side of the atoms; the latter declaring that to him it seemed probable "that God in the beginning formed matter in solid, massy, hard, impenetrable, movable particles . . . and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear or break in pieces, no ordinary power being able to divide what God Himself made one in the first creation." And, finally, John Dalton, the greatest of the "Atomists," as those who upheld the grained structure theory of matter once were designated, placed the atomic hypothesis on a firm foundation by showing how it might be applied to the elucidation of chemical phenomena.

Let it be admitted that the matter of the universe is composed of minute, invisible particles, which have never been broken down or destroyed in the various physical and chemical changes to which we have subjected them, except conceivably in certain special cases connected with radio-active change. Let it be admitted, further, that there are as many kinds of atoms as there are chemical elements, say, about eighty, and that the weight of the atom of each element differs from that of the atom of every other element known to us. Then the question is, How can we compare the weights of these eighty different kinds of atoms?

Dalton himself made courageous attempts to solve this problem. But he was at a great disadvantage. He was able to give us reasons for thinking that the weights of the atoms of different elements are unequal, but to weigh them correctly was not yet possible in his time. In some cases he was able to state, approximately, the proportions in which the better known elements combine. He knew, for example, that in water one part of hydrogen is united with eight of oxygen.[1] But Dalton and his colleagues could not tell us whether these proportions of hydrogen and oxygen correspond to the union of one atom of hydrogen with one atom of oxygen, or to the union of two atoms of hydrogen with a single atom of oxygen, or to some other more complicated arrangement. And thus for a long time but little progress was made, except perhaps in Italy, where the delicate perceptions of Avogadro enabled him, as early as 1811, to recognise the existence of a silken thread which might have guided us into the right path many years before most of the chemists actually walked there.

Is it not plain that if all matter consists of minute, indivisible particles which conform to a very limited number of types, and if all the thousands of compounds known to chemists are produced by the joining together of these atoms in various numbers, then there must be two distinct classes of particles to be considered—first, the atoms, and, secondly, various groups of atoms; each particular group probably corresponding to a given element, or to a given compound substance? In these latter groups, the *molécules intégrantes* of Avogadro as distinguished from the *molécules élémentaires* or atoms, we have the molecules of the modern chemist.

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[1] Dalton's value was somewhat lower than this.

The distinction, to us so obvious, between the atoms and molecules of the elements was recognised by Avogadro and Ampère at a very early stage. But it so happened that in their hands it was fruitful only when applied to the gases. And thus a quarter of a century elapsed before their ideas on this subject, and before Avogadro's famous hypothesis on the constitution of the gases, which teaches us that "in all elastic fluids"—gases—"observed under the same conditions the molecules are placed at equal distances," bore their predestined fruit in the hands of his eminent successor, Jean Baptiste André Dumas and those who followed him.

As it would be impossible within the limits of twenty or thirty pages to give even a passing glance at the individual labours of the small army of chemists who have struggled with the problem of weighing the atoms, we will now drop the historical details of our subject, and turn our attention to its broader aspects.

Let us see exactly where we stand. According to the teachings of Avogadro, Ampère, Dumas, and the modern chemist, matter exists in two distinct states of subdivision. First, there are the atoms, which as far as we know are quite indivisible by chemical means. Secondly, there are groups of atoms held together by some kind of attraction, and constituting the larger particles called molecules; a definite group corresponding to each element and to each compound. The distinction between elementary and compound molecules in terms of the atomic hypothesis being this, that in each of the former all the atoms are similar, and that the molecule may even consist of a single atom, whilst the molecules of compounds must contain, every one of them, atoms of at least two different kinds. Then, in addition, Avogadro's hypothesis teaches us that equal volumes of gases, if measured at the same temperature and pressure, contain equal numbers of molecules. This last statement is not absolutely true, but it approaches the truth sufficiently for our purpose. It holds equally when applied to elementary gases like oxygen and hydrogen and to compounds like steam, which is composed, as we know, of oxygen and hydrogen, provided that the steam is really in the gaseous state, that is, if it is at a sufficiently high temperature.

Now, what Avogadro's hypothesis does for us is this. It enables us to get round the difficulty created by the excessive minuteness of atoms and molecules. Because if equal volumes of two gases contain equal numbers of molecules, then from the behaviour of these equal volumes, or of any other known volumes of these gases, when they react with one another, we can draw conclusions as to the behaviour of single molecules. For example, under suitable conditions two volumes of the gas hydrogen will combine with one volume of oxygen, and produce two volumes of water in the form of steam. It does not matter what volumes are taken; they may be cubic inches, pints, gallons, cubic centimètres, what you will, provided that they correspond to the proportions mentioned above.

Now suppose that in a given case the one volume of oxygen contained one billion molecules of oxygen. Then would it not follow from Avogadro's hypothesis that the two volumes of hydrogen contained two billion molecules of hydrogen, and that the two volumes of steam produced by their combination contained two billion molecules of steam?

But if this is so, one billion molecules of oxygen will unite with two billion molecules of hydrogen and yield two billion molecules of steam; or, dividing each of these numbers by one billion, we find that one molecule of oxygen will unite with two molecules of hydrogen and produce two molecules of steam.

Thus, the hypothesis affords us a bridge, as it were, by which we can pass from large volumes of gases which we can handle to the minuter molecules, which individually are invisible, intangible, and only to be clearly conceived, in fact, by the exercise of a well-trained imagination.

Before we proceed to apply the teachings of Avogadro, in our attempt to solve the problem of weighing an atom, there is one other illustration which will help us to realise its value. It is easy to see that in each molecule of a compound there must be at least one atom of each constituent element, and, accordingly, that molecules of compounds must be made up of two, three, four, or some larger number of atoms. But it is by no means equally easy to form an opinion about the molecules of the elements; to decide, for example, whether these consist of single atoms or of pairs, of triplets, or of yet more complex groups. Now this is a question of considerable importance.

We know, as has been explained already, that one volume of oxygen will combine with two volumes of hydrogen and produce two volumes of steam, or, substituting as before molecules for volumes, that a molecule of oxygen will unite with two molecules of hydrogen and yield two molecules of water in the form of steam. This tells us just what we want to learn. For since there must be at least one atom of oxygen in each of these two molecules of water—that is, two atoms of oxygen in the two molecules of water taken together — it is clear that the molecule of oxygen from which they were produced must itself have contained at least two such atoms, for it would be inconsistent with the whole body of chemical knowledge to suppose that a single atom of any kind is created in the course of any chemical change. By similar experiments, supplemented by similar reasoning, we can arrive at the constitution of other elementary molecules, and we find that while hydrogen molecules and many others are diatomic like oxygen, others again are differently constituted, some, *e.g.* ozone, the more active phase of oxygen, being composed of three atoms, others of four and so on; whilst some, for example quicksilver and argon, have molecules which are composed of single atoms.

Before we may hope to follow the processes, simple as they are in principle, involved in weighing an atom, we have still to gain a really definite idea of what it is we want to weigh. At present we are too nearly in the position of the chemists of a century ago, for it was just the want of a really definite and correct idea of an atom that made it so difficult for Dalton and the chemists of his day, not only to fix atomic weights, but even to argue with one another comfortably about them. Let us, then, endeavour to throw our notions into a more precise form.

Every one is familiar with the element carbon, which exists in the forms of soot, diamond, and black lead. Most of us know that carbon is one of the most important of all the elements; that its compounds are vastly more numerous than those of any other single element, and perhaps more numerous than those of all the other elements taken together ; that it is one of the chief components of the tissues of all animals and all vegetables. And some, perhaps, are aware that many of the carbon compounds are gases, or become gaseous at high temperatures, and that, consequently, this element lends itself well to our purpose. Therefore let us try to answer the question, What do we mean, exactly, by an atom of carbon ?

Since atoms have never yet been divided in ordinary cases of chemical change, and since they unite to form the larger and more complex individuals called molecules, one thing seems quite clear. If we can discover what is the smallest quantity of carbon that is present in any one of the molecules which contain carbon, we shall have a quantity which must correspond to the weight of one, two, or more atoms of that element —a weight which may be greater than that of an atom of carbon, and, if so, must be an exact multiple of its atomic weight, but which cannot be less than the weight of a single atom of carbon, since no molecule can contain less than an atom of any constituent element. These considerations carry us a step forward, and plant our feet on comparatively firm ground, but they leave us in need of a standard of reference.

When the earliest attempts to compare the "weight" of the earth with the weights of the heavenly bodies were made, it was found impossible to express the values calculated from astronomical observations in accordance with any of the common standards, such as the gram, the ounce, or the pound. Accordingly, the earth itself was adopted as the standard, and was said to have the density 1; the density of the sun, which is one quarter as great as that of the earth, being expressed by the figures 0.25, that of Venus as 0.9, and so on. We meet with exactly the same difficulty in the case of the atoms. It is true that it is possible to make shrewd guesses at, or perhaps I might say to estimate, the weights of certain atoms, and one of these estimates puts the weight of an atom of hydrogen, for example, at about the forty-million-million-millionth part of nine one hundred-thousandths of a gram, a gram being 15½ grains; but for several reasons it is thought better to take an atom of hydrogen as our standard, and, for the sake of simplicity, to say that this weighs 1. Hydrogen being chosen because its atoms are the lightest, although there are certain practical objections to the selection.

Now, suppose we were to obtain and analyse samples of all the compounds formed by hydrogen with other elements, and also samples of every compound containing carbon, and in this way ascertained the respective proportions of hydrogen and carbon in 100 parts of every compound. And suppose, further, that we determined also the weight of the molecule of every one of these compounds. Then, by doing a number of sums in proportion, we could find what proportion of hydrogen is present in a molecular weight of each compound containing hydrogen, and what proportion of carbon is present in a molecular weight of every compound containing carbon. If we did all this, I think we should discover the smallest quantity of carbon in a molecular weight of any carbon compound to be twelve times as great as the smallest quantity of hydrogen in a molecular weight of any compound of hydrogen, and I express this opinion by saying that atoms of carbon are twelve times as heavy as atoms of hydrogen.

In practice, however, we have to content ourselves with something far less exhaustive than the tremendous research outlined in the previous paragraph. There are thousands and thousands of compounds containing carbon and hydrogen. We do not know the composition of all these compounds, and we do not know their molecular weights in every case, and therefore we must be satisfied with some scheme far less ambitious than the ideal one which I have put before you. We might, for example, find the composition and molecular weights of as many compounds of carbon and of hydrogen as circumstances permit, and then, because we can do nothing better, take for the weight of an atom of carbon the smallest quantity of carbon we have found in a molecular weight of any compound containing carbon, compared with the smallest quantity of hydrogen found in a similar manner in a molecular weight of any compound containing hydrogen. The atomic weight of carbon thus arrived at is 12. If we extend the idea of an atom as thus defined to the other elements, you will see we may say that the atomic weight of any element is the smallest weight of that element yet discovered in any molecule containing it compared with the atomic weight of hydrogen ascertained in a similar manner and taken as 1. In short, the atomic weights of the chemists give us the relative weights of the atoms. They tell us that carbon atoms are twelve times as heavy as hydrogen atoms, oxygen atoms sixteen times as heavy, and so on ; but since we do not know definitely how many hydrogen atoms go to make a gram, we cannot give any similar information about the weights of the other atoms either. We are not, in fact, quite so far advanced in the process of weighing the minute atoms as in that of weighing the great globe, the earth. Nevertheless, even in this direction, as has been said already, a certain amount of progress has been made.

I hope my readers have gained now in a general way a tolerably distinct idea what we mean by the weight of an atom, and that they realise the part played by Avogadro's hypothesis in fixing these weights. If equal volumes of two gases contain equal numbers of similar molecules, is it not clear that the weights of these equal volumes must be proportional to the weights of the individual molecules which compose them; and that if we desire to learn the compositions of the molecules of the two substances, we have only to analyse equal volumes of them in order to discover what we wish to know?

Guided by these considerations, we see that to ascertain the relative atomic weight of an element, we must analyse as many compounds of the element as possible, deduce the molecular weights of these compounds from their densities[1] in the gaseous state, as indicated by the famous hypothesis of Avogadro ; and then take as the atomic weight of the element the quantity present in a molecular proportion of that compound which contains the element in the smallest proportion. The actual process of weighing an atom is not, truly, by any means so simple as my words suggest. There are two serious sources of error. First, it is not easy, though it has been done in some cases, to compare the weights of equal volumes of gases very exactly. Hence molecular weights based upon the densities of gases are apt to be less close to the truth than we could wish. Secondly, much depends on the chemist including among the compounds he analyses that particular compound which contains the element he studies in the smallest proportion ; on his being able to prepare that compound in a highly purified state ; on its being a substance which lends itself to exact analysis, and also one whose vapour density can be determined. Thus there are many pitfalls, and failure, as you will perceive, on any single point may be fatal to the final result.

I have explained how difficult it may be to follow precisely the line of work suggested in the earlier parts of this essay. Even if this were not so, however, we should still seek light from other directions. In science, as in the law courts, we are compelled, sometimes, to rely upon the evidence of a single witness—that is, on a single fact. When two facts seem to be in conflict, we may be driven to decide which is the more credible of the two. But we prefer, of course, to have independent confirmatory evidence before us, and as much of it as possible. Hence ever since the problem of weighing atoms was seriously attacked, chemists have been on the look-out for new methods. We want, first, further methods of weighing molecules, so that the ideas expounded above may be applied to the case of substances which have not been made gaseous—that is to say, to cases which are not covered by Avogadro's hypothesis; and, secondly, science demands further methods of weighing atoms, in order that we may control the results obtained by working along the lines already suggested.

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[1] The weights of equal volumes of gases are known as their densities.

Fortunately, as we shall see, atoms and molecules have other measurable qualities besides mass, and thus the resources we seek are at our disposal. These resources, in fact, though not exactly abundant, are sufficiently varied and extensive to compel us here to restrict our attention to a few illustrations. First, let us consider the case of the molecules.

Avogadro has shown us how to deduce the relative weights of the molecules of gaseous substances from their densities, but unfortunately many substances cannot be made gaseous. Raoult, the French physicist has come to our aid here, and has taught us how to weigh the molecules of substances when they are dissolved in water or other solvents.

Unfortunately, again, some substances, when heated to the point at which they turn into vapour, do not merely undergo a physical transformation like that which occurs when water is converted into steam, but for the time being are destroyed—that is, converted into new things altogether—with the result that if we calculate the weights of their molecules from their densities we draw completely wrong conclusions. Chemists have learnt how to detect these substances, however, and have invented chemical methods of weighing molecules, which can be applied to these and other cases of a similar kind. These two examples are very far from sufficient; they do not exhaust our resources, nor do they fully cover the ground. But they will give a good idea of these resources, and, the reader's time and probably his patience being limited, they must suffice.

Raoult's beautiful method of weighing molecules is based on the freezing-points of solutions. Every one knows that sea water freezes much less readily than river or spring water. This is due to the solid matter which sea water contains. And it is a curious and interesting fact, speaking generally, that dissolving a little foreign matter, such as sugar, in pure water not only lowers the freezing-point of the latter, but reduces it in such a way that the effect produced is very simply related to the molecular weight of the solid dissolved, except in the case of solutions which conduct electricity. It may seem absurd, but nevertheless is true, that by observing the temperature at which a dilute solution of sugar freezes, a chemist can determine the weights of the molecules of sugar compared with the weight of an atom of hydrogen. The process cannot even be said to be very difficult, for quite respectable results can be got by capable schoolboys after a little practice. All that is wanted is a chemical balance, a delicate thermometer, a few glass tubes and basins, some ice, and the power to use them. Nor is the idea of the method difficult to follow. If you take half-a-dozen suitable compounds, all of known molecular weight; dissolve weighed quantities of each separately in known quantities of water, so as to obtain dilute solutions of known concentration; observe the temperatures at which these solutions freeze, and then, from your results, calculate the freezing-points of a set of similar but stronger solutions, containing respectively a molecular weight in grams[1] of each solid to one hundred grams of water, you will find in every case that the calculated freezing-point is not far distant from – 19° C. There are exceptions to this rule, but these can be accounted for; and thus, if we can determine the number of grams of a given substance which must be dissolved in 100 grams of water, in order to produce a solution which will freeze at – 19° C., we shall have its approximate molecular weight, unless the substance is a member of one of those classes which are known not to conform to Raoult's rule. Other solvents may be employed in place of water, and other physical properties—

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[1] The molecular weight of "common salt" is 58.3; its molecular weight in grams accordingly is 58.3 grams.

e.g. the temperatures at which solutions of known concentrations boil—can also be made use of, but we must not dwell upon these here.

Soon after Dumas re-directed attention to the methods of applying Avogadro's hypothesis to the weighing of molecules, it was found that in certain cases it led to results which chemists were quite unable to accept. This brings us to a chemical method of weighing molecules.

The vapour density of sulphuric acid suggests that its molecule must be forty-nine times as heavy as an atom of hydrogen. Now no chemist can admit that this is correct.

When sulphuric acid is mixed with an alkali, such as soda, in certain proportions, its acid qualities are destroyed—it is, as we say, neutralised—and if we analyse the salt thus produced we find that the hydrogen of the original acid is gone and the metal sodium reigns in its stead. If, however, we vary the amount of soda used, if we take half as much soda as is necessary to neutralise a given weight of acid, or twice as much, or one-third as much, and so on, we discover, sooner or later, that we can get two distinct salts from sulphuric acid and soda, and no more. We find, moreover, that in one of these salts all the hydrogen of the acid is replaced by sodium, in the other only half. Now, if the hydrogen in the molecules of sulphuric acid exists there in the form of indivisible atoms, as the atomic theory asserts, does it not follow, since we can only expel this hydrogen in two stages, first one half and then the second half, that each molecule of the acid must contain two atoms of hydrogen, no more and no less? But if this is so, if each molecule of sulphuric acid contains exactly two atoms of hydrogen, then that weight of acid which contains these two atoms—that is, for practical purposes, two parts of hydrogen—will be its molecular weight. Analysis shows that ninety-eight parts of sulphuric acid contain two parts of hydrogen, and therefore the chemists say that its molecular weight is 98, not 49.

Perhaps you may ask, Does not this force us to abandon this application of Avogadro's hypothesis? No, it does not do this. It only warns us to take care that we do not apply the hypothesis to the case of a substance, like sulphuric acid, which splits up when heated. And as, usually, it is not very difficult to detect such substances, Avogadro's hypothesis stands unshaken.

And now we must consider, in conclusion, one or two other characteristic properties of the atoms which we can apply in the operation of weighing them. One of the most remarkable and important of these, which can only be mentioned in passing, is connected with the shapes of the crystals into which they enter; another of equal importance, and more easy for laymen to follow, is their capacity for heat.

There is a familiar experiment in physics which consists in making several balls equal in weight but composed of different metals equally hot by placing them in boiling water, and then quickly transferring them to a slab of wax. When this is done the metallic masses sink into the wax at very different rates, some melting much wax and making large holes, others melting little wax and making holes which are smaller. This is due to the fact that equal weights of different metals take up unequal quantities of heat when their temperatures rise through equal numbers of degrees, say, for example, from 0° C. to 100° C., and therefore, in accordance with a well-known principle, give out unequal quantities of heat during the subsequent process of cooling. About the year

1819 it occurred to Dulong and Petit to consider the effect of taking, instead of equal weights of the elements, atomic weights, or rather quantities proportional to their atomic weights. Thus, the atomic weight of iron being 56 and those of copper and tin 63 and 118, they did not study the behaviour of 1 gram or of 10 grams of each metal, but that of 56 grams of iron, 63 grams of copper, and 118 grams of tin. The result was very illuminating. They found that the quantity of heat required to raise an atomic proportion of a metal from 0° C. to 100° C., or through any corresponding range of temperature, was nearly the same in each case. Here, then, we have a new and splendid criterion to help us to fix atomic weights. The atoms of the elements have, approximately, equal capacities for heat. If a certain quantity of heat is required to raise an atomic proportion, say, 56 grams, of iron from the freezing-point to the boiling-point of water, then an approximately equal quantity of heat will be needed to raise an atomic proportion of any other metal through an equal range of temperature. When once this quantity of heat has been fixed, then to find the atomic weight of a new element we have only to ascertain by an experiment, not a very easy one however, how much of the new element absorbs this quantity of heat in passing from 0° C. to 100° C. This is, in effect, the famous rule of Dulong and Petit. The results obtained by its aid are not very exact, because it is not easy to carry out the necessary experiments under suitable conditions. But this does not matter very much, for we have the means of adjusting them. A more serious defect lies in the fact that whilst the rule applies well to the metals, which form the majority of the elements, it does not do equally good service in the case of such elements as carbon, silicon, and boron. But here, again, forewarned is forearmed. We have only to be cautious when we study non-metals, and no harm will befall. Dulong and Petit's rule has done chemistry great service.

One more illustration and I have done. From early days chemists have been in the habit of arranging many of the elements in groups or families according to their resemblances. By studying these groups they gradually recognised signs that there exists some connection between the properties and atomic weights of the members of these groups, and in 1864 an Englishman, Mr. J. A. R. Newlands, was on the verge, as it seems to us now, of fully discovering the law subsequently worked out by Professor Mendeléeff in Russia and by Professor Lothar Meyer in Germany, now widely known as the Periodic Law, which enabled the former to predict the existence of a number of elements and to foretell their chief chemical and physical properties *and their atomic weights.* According to this law the properties of the elements vary periodically with the weights of their atoms, so that if the elements are arranged in the order of their atomic weights similar elements recur at somewhat regular intervals ; the eighth element resembling the first, the ninth resembling the second, and so on, which enables us to foretell the properties of an element if we know its atomic weight, or to foretell its atomic weight if we are acquainted with its properties. This state of affairs seems unlikely to be the result of mere accident—the chances against that are too great—and thus it affords us a distinctly useful means of checking atomic weights selected upon other considerations.

The Periodic Law by itself does not enable us to make close determinations of the weights of atoms. But this causes no serious difficulty. For in every case, in practice, the actual selection of the atomic weight of an element is controlled by the fact that, as any given atom in combining with hydrogen must unite with one, two, or more atoms of hydrogen, the true atomic weight of the element must be an exact multiple of the quantity of it which will combine with a single atom or one part of hydrogen. This, however, brings up another subject—viz. the methods of fixing "the combining numbers" of the elements, which is far too big a matter to touch here.

SOME ASPECTS OF THE NEW CHEMISTRY

WHEN the Editor of the *Cornhill Magazine* proposed, some years ago, that I should write an essay on the "New Chemistry" I was, I must confess, a little disconcerted, for modern chemistry, like a crystal, presents many faces, and it seemed hopeless to attempt to deal with all of them, or even with the chief of them, within the limits of such an essay.

But after all, though the lines which divide the faces of a crystal are sharp, and real enough, they do not go even skin-deep. And moreover, the crystallographer does not confine himself to examining and measuring the faces of his crystals, or to studying the relations which these faces bear to one another. On the contrary, he tries to go to the root of the matter; considers the character of the stuff of which the crystals are composed, and tries to picture to himself the possible forms and relations of the ultimate particles of which they are built up, which must really determine the wonderful forms they exhibit.

And so also the chemist. He too, amidst efforts to clear up the mysteries of organic chemistry, of fermentation, of the chemical relations of plants and animals, the nature of flame, and all the rest of it, never ceases his search after light and leading about the ultimate structure of the matter which reveals itself in such myriad forms, and about the force or forces which control and determine the marvellous transformations that matter undergoes in nature and in the laboratory. And he is right. For, important as the rest may be, this is the heart of his subject. This is chemistry, as distinct from any and every department of chemistry.

The new chemical theory—which by the way is essentially an electrical atomic theory—is the outcome of the labours of a century. Its foundations were laid nearly a hundred years ago by Nicholson and Carlisle, who discovered the action of electricity on acidulated water; by Dalton of Manchester, the author of the Atomic Theory; and by Sir Humphry Davy, Professor of Chemistry in the Royal Institution. Their work was continued with brilliant success by Faraday, and the new theory has only taken its present form at the hands of Arrhenius and his colleagues during comparatively recent times.

To John Dalton we owe the "Atomic hypothesis." According to this, every definite chemical compound, such as water, chalk, sugar or sand, owes its constancy of composition to the fact that it consists of minute particles called molecules, each of which is built up of a fixed number of still smaller particles called atoms, these latter being indestructible and of fixed weight, and there being as many kinds of atoms as there are elements in each molecule of every compound.

According to this hypothesis, now usually known as the "Molecular atomic hypothesis," matter is by no means so homogeneous as it often seems to the eye, but on the contrary has always a grained structure, and if it could be examined with a sufficiently powerful magnifier would be found to present the appearance of sand or sandstone rather than that of a jelly or a piece of ivory as these appear to the naked eye. The particles discovered by our magnifier would not, however, be the atoms of Dalton, but molecules, and would be built up, according to what has been said, of yet smaller and chemically indivisible particles, the chemical atoms. Moreover, the molecules would not be still, but in a state of motion.

Matter may be resolved by suitable means into a few simple forms known as the elements. There are about eighty of these elements, and we do not know much about their real nature. We only know that all the forces at the command of the chemist hitherto have failed to reduce them to simpler forms. According to our theory the atoms of each element, whilst exactly alike in every respect, are quite unlike those of every other element, but all, with the exception perhaps of atoms of helium and the atmospheric elements, argon, neon, krypton and xenon, exhibit a marked disposition to attract or combine with other atoms. So that even in the case of the elements themselves in the free state single atoms are not often met with, but almost always molecules consisting of two or more atoms united; elementary substances being those in which the molecules consist of similar atoms, and compounds those in which the molecules contain dissimilar atoms.

It is supposed, of course, that both the atoms and the molecules are exceedingly small. It is difficult, indeed, to give a clear idea of the minuteness of their dimensions, or to express their weights by reference to any of the ordinary standards. But some ideas on this subject will be gained when I say that there are living organisms, which must themselves be built up of many molecules, so small that half a million of them could lie in a single layer on the surface of a postage stamp; and again, that it has been calculated that about 20 million-million-million, or 20,000,000,000,000,000,000, molecules of a gas could be contained by a vessel about as big as a medium-sized die[1]; that nearly every such molecule would consist of two or more atoms; and that if the gas were reduced to the liquid or solid form, as every common gas can be, these 20 million-million-million molecules could be packed into a vessel many many times smaller, small though this may seem to be.

It may be added that in spite of difficulties the weights of the atoms of the better known elements have been compared with such success that, though it is still impossible to state exactly how many atoms of hydrogen would weigh a gram, we are able to say with some confidence that an atom of oxygen is sixteen times as heavy as an atom of hydrogen, an atom of sulphur thirty-two times as heavy as one of hydrogen, an atom of quicksilver two hundred times as heavy, and so on. In short, that we are able to express the weights of the chief atoms in terms of the weight of an atom of hydrogen taken as one.

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[1] The total volume of eight such vessels would be less than a cubic inch.

Dalton's theory quickly attained a great success, in spite of some criticism at the earlier stages of its career. Probably this was due in no small degree to the fact that in recommending it to the notice of the scientific world, he illustrated his ideas by means of a system of symbols or pictures of the atoms which enabled him, as it were, to show on the blackboard the combinations, separations, and other changes in which they play a part. In the hands of his successors this system of symbols has been transformed into a kind of chemical algebra that has been of incalculable value in the science.

Apart from his theory, and apart from his other contributions to science, John Dalton will for ever remain one of the most interesting figures among his scientific colleagues. Born in Cumberland in 1766, of humble parentage, with few educational advantages, without influence, and with no other means than those he could earn by teaching science and mathematics to such pupils as he could find in a provincial town, he raised himself before he died, in Manchester in 1844, to a position of the greatest dignity and consideration among the leaders in science.

Dalton was born at Eaglesfield, not far from Cockermouth, as said above, in 1766, his father, a member of the Society of Friends, being a hand-loom weaver in that place. He had exceptional good fortune in coming under the care of an able teacher, Mr. Fletcher, and in making the early acquaintance of Mr. E. Robinson, a Quaker gentleman of scientific ability and tastes, who lived in the neighbourhood. He is said not to have been a brilliant or quick boy, but he was certainly an unusual boy, as will be recognised when I mention that at the age of twelve he started as schoolmaster at Eaglesfield, apparently as a means of livelihood, and continued at the work for two years, in spite of such difficulties as were sure to occur with his older pupils, some of whom were many years senior to himself. The teaching profession seems always to have attracted Dalton, for he continued to follow it almost to the end of his life; first at Kendal and afterwards in Manchester, and this upon so simple a footing that when, in 1826 and in the height of his fame, M. Pelletier the *savant* came expressly from Paris to visit

Dalton he found him, not lecturing, as he had expected, to a large class in a lecture-hall, but teaching a single small boy to cipher on a slate in a small room in a small house in a back street. His merits would seem to have been recognised abroad sooner than at home, for he was made a corresponding member of the French Academy several years before he was elected into the Royal Society, and in its early days Davy was opposed to his atomic theory. In later years, however, his merits were fully recognised, and in 1833 a pension—albeit a small one—was conferred upon him. But pensions for men like Dalton were very difficult to get in those days. They are not easily obtained for them even now.

About the time that Dalton was working out his atomic theory at Manchester there came to the then newly founded Royal Institution in Albemarle Street—at the invitation of its founder, Count Rumford—a young chemist, then only in his twenty-second year, whose discovery of the remarkable properties of "laughing gas" at the Pneumatic Institute in Bristol had already attracted widespread attention. This was Humphry Davy, "poet and philosopher," the discoverer of the metals sodium and potassium; the first to suggest the idea "that in all cases of chemical decomposition the elements might be related to each other as electrically positive and negative," and, above all, the discoverer of Faraday, his greatest successor in the field of electrochemistry. Davy's career forms in many ways a striking contrast to that of Dalton. His early years were a time of difficulty, owing to the misfortunes of his father, who came of a respectable yeoman family, which was established near Penzance, and was at one time in fairly comfortable circumstances. But showing an early genius for chemistry, and possessing an alert and engaging personality, Davy, more fortunate than Dalton, soon found his chance. He was made, as I have already said, Professor at the Royal Institution at an age when most young men of talent are at the University, and after dazzling London with his discoveries and his discourses, became President of the Royal Society at forty-two. He died—burnt out, it is to be feared—in his fifty-first year. But it is time to dismiss the chemists and return to their science.

I must remind my readers that it has been a cardinal point in the atomic theory that atoms as a rule attract one another; that when one solitary atom finds itself in the presence of another, these two almost always unite. If the atoms are similar atoms the resulting molecule is still elementary. If the atoms are dissimilar the product is a compound, the strength of the attraction and consequently the stability of the molecule produced being greater, it would seem, in proportion as the atoms concerned are themselves unlike. Very dissimilar atoms, such as those of the metals on the one hand, and those of such active non-metals as chlorine or oxygen on the other, may produce combinations which will bear, unaffected, exposure to the temperature of the hottest furnace.

The nature of this attraction, which the elements exert upon one another, and which has been called chemical attraction or chemical affinity, is very difficult to learn. But we believe that the amount of action in any given case depends partly on the masses of the acting bodies and partly on their affinity. So that if the masses are made chemically equal in any case, chemical affinity alone determines the result obtained, provided that physical complications due to the solubility, insolubility, &c., of the bodies concerned can be prevented from influencing the result.

Davy's attention was, I believe, first turned to electro-chemistry by the great discovery, made on April 30, 1800, by Nicholson and Carlisle, that water can be resolved into its elements by means of the electric battery. This led him to work at electro-chemistry even while still at Bristol, and induced him, when the greater resources of the Royal Institution were at his command, to set up the great battery which he used in his subsequent researches. The actual results he attained, such as the discovery of sodium and potassium, were in themselves of the greatest importance, whilst his suggestion that the elements in a chemical decomposition might be related to one another as electrically positive and negative, though not universally accepted at the time, has been fruitful in the highest degree in the hands of Faraday and of his successors. It will be seen how highly these results and speculations were valued by the *savants* of his own day, when I mention the fact that in 1807 a committee of the French Institute, to their undying honour, awarded to Davy the medal founded by Napoleon "for the best experiment which should be made in the course of each year on the galvanic fluid," although England and France were at that time at war, and the two countries were animated by the most hostile feelings towards each other.

Napoleon, it would seem, did not rise to the height of the occasion, like the French *savants*. For "all the members of the Institute got for themselves was," as Miss Edgeworth says, "a rating all round in Imperial Billingsgate." But alas! there were those in England who said that patriotism should prevent the acceptance of this splendid award.

Davy taught that all combinations and decompositions produced by electricity may be referred to the laws of electrical attractions and repulsions; that chemical and electrical attractions are produced by the same cause acting in the one case on particles and in the other on masses, and "that the same property under different modifications is the cause of all the phenomena exhibited by different voltaic combinations." But though these views were both precise and, as the event showed, prolific, they were, as Whewell has pointed out, "conjectured rather than proved," and it remained for Faraday to establish their truth by one of the most splendid series of experimental researches ever carried out in any age or any country.

In 1806, when Davy's great memoir was published, Faraday, then about fifteen years of age, was apprenticed to Mr. Riebau, a bookbinder and stationer in Blandford Street, and was spending such small leisure as he could command in reading Marcet's "Conversations on Chemistry" and the electrical treatises in the "Encyclopædia Britannica," and "learning to think," as he says, from Watts "On the Mind." A few years later, 1812, he attended some of Davy's lectures, and soon after had the good fortune to become his assistant, and subsequently his successor.

Besides discovering benzene, doing work of the greatest importance on magneto-electric induction, &c., and on the liquefaction of the gases, Faraday, like Davy, turned his attention to the study of electro-chemistry with most brilliant results. In one of his earlier contributions to this department he proved the identity of voltaic and animal electricity with that produced by the electric machine. And subsequently, having worked out a method for measuring electrolytic action, he was able to show that the chemical power of an electric current between two poles is in direct proportion to the quantity of electricity which flows between them; and further, that the proportions of the elements deposited at the poles by the current are identical with their atomic weights, as fixed from purely chemical considerations: thus making it clear beyond the possibility of doubt that chemical and electrical actions are indeed indistinguishable, as Davy believed, and, if not identical, must at any rate have a common cause or origin.

Faraday's conclusions, founded as they were on exact observations and clear ideas, were quickly accepted. But the precise connection between this theory of chemical action and Dalton's theory of atoms was not yet understood; the marriage of these two parts of the "New Chemistry" had still to be effected. Let us now see how this has been done in more recent times.

According to the modern Kinetic hypothesis, we must suppose that every liquid consists of immense numbers of minute particles held together by an attractive force, but still free to move, and in fact always in motion, gliding about amongst one another. These particles, it must be remembered, are not the atoms of the chemist, but molecules built up of atoms held together by an attraction which Faraday has shown to be indistinguishable from electricity.

If a current from a galvanic battery is conveyed to various liquids by means, let us say, of two carbon rods, it is found that the liquids fall into two classes according to their behaviour. Many liquids when treated in this way behave like pure water; that is, they remain unaffected by the electricity. But solutions of acids, alkalis, and salts in water, and some of these substances in the fused state, are obviously altered. Let us suppose, for example, that we tried the experiment, using a solution of hydrochloric acid. Then we should find that a green malodorous gas of great bleaching power, chlorine by name, would appear at the rod called the anode, and a colourless, odourless, very light and combustible gas, hydrogen, at the kathode, the chlorine remaining dissolved in the water at first, but escaping in volumes equal to those of the hydrogen as soon as the water is saturated with it.

It makes little difference whether the acid used is strong or weak, or whether the current be feeble or intense, provided that the proportion of acid be not unreasonably small, and that the current reaches a certain minimum. In every case the same gases will be produced in the same relative proportions, if all interfering circumstances are duly avoided. The action of the electricity on the liquid will continue until all the acid is destroyed, but no longer. And if the two gases are collected, mixed, and inflamed by a spark, they will at once regenerate hydrochloric acid gas which is in every way similar to that originally used in preparing the solution employed for the experiment. Thus by means of electricity we can overcome the attraction between the atoms of hydrogen and chlorine; break up the molecules of hydrochloric acid, and transport their constituent atoms to the two poles or electrodes. And, though these electrodes may be an inch or more apart, we shall see no signs of the gases passing between them. How can this be? When millions of molecules of hydrochloric acid deposit millions of atoms of chlorine at one carbon rod, how can the millions of atoms of hydrogen pass across the intervening space and appear at the other rod without in any way, so far as we can see, disturbing the liquid through which they pass?

Several hypotheses have been put forward to account for this remarkable phenomenon, but we need only concern ourselves with one of them, viz. the hypothesis of Clausius, which in a modified form, known as the "electrolytic dissociation hypothesis," now holds the field.

If we think about our solution of hydrochloric acid in water, and picture it to ourselves as it appears to the mind of a chemist, we shall perceive that it is likely to consist of two kinds of particles or molecules. There will be molecules of water, and gliding about among them a number of molecules of hydrochloric acid. If we concentrate our attention upon these latter molecules, and regard them simply from the point of view of the Daltonian atomic theory, we shall conceive each molecule of hydrochloric acid to consist of two atoms, one of hydrogen, which might be represented by the figure ⊙, the other chlorine, which might be indicated by the symbol ●, these being closely bound together by chemical attraction and therefore written ⊙●. Now so long as the solution is not under the influence of the battery there is nothing in its more obvious properties to suggest that the atoms of chlorine and hydrogen are not as closely bound together as this formula suggests, and as they seem to be when the same compound is met with in the gaseous state. No sooner do we dip the electrodes of the battery in the liquid, however, than, as said above, the hydrogen atoms[1] appear in millions at the kathode, and the chlorine atoms in corresponding numbers at the anode, and at a great distance from one another, though a moment before they must have been in closest contact.

Faraday and the chemists of his period supposed that the constituents of the molecules of hydrochloric acid, and other electrolytes, were held together strongly in the molecules by chemical forces, and they were surprised to find that the best conductors — that is, the substances most easily decomposed—were just those compounds in which the atoms were believed to be most firmly bound together. But after it was shown, in 1853, that the very smallest electro-motive force is sufficient to cause a current in an electrolyte—that is, to decompose some of it into its constituents, or in other words to split up some of its molecules—it was seen that the older view must be abandoned; "that the radicles of the electrolyte cannot be held together by a force of finite value," but that exchanges among the atoms must constantly be taking place in the solution, and that possibly a few of these atoms, or *ions* as we shall call them in future, might actually be free in certain cases.

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[1] United to form molecules of hydrogen, however.

According to this view, which we owe to Clausius in the first instance, we must give up the idea that when hydrochloric acid is dissolved in water the solution consists solely of molecules of acid and molecules of water, and suppose instead that some of the molecules of the acid are broken up into their ions, and that each of these carries a charge of electricity, a positive charge in the case of the hydrogen ions (hydrions), a negative charge in the case of the chlorine ions (chlorions). When they meet, no doubt these ions recombine. But this would not reduce the number of free ions in the solution permanently, for such losses would constantly be compensated by the liberation of fresh ions at the cost of the acid.

If this new hypothesis, thus broadly stated, be accepted, the phenomena of electrolysis at once become more comprehensible. For under the influence of the voltaic battery the ions of hydrogen and chlorine, in accordance with the ordinary laws of electrical attraction, would tend to move with their charges of electricity to the kathode and anode respectively, and there, being discharged, would unite and become molecules of ordinary hydrogen and chlorine once more.

It was not at first thought necessary to suppose that any considerable quantity of the hydrochloric acid, or other electrolyte, would be dissociated into ions at a given moment, for it was considered that the liberation of the ions would be continuous, and there would be a constant supply of fresh ions to take the place of those removed under the influence of the battery. Indeed Clausius admitted that the presence of many free ions at any moment was unlikely, in view of the fact that they could not be recognised by any methods then known to chemists.

But with the growth of our knowledge of physical chemistry it has become necessary to modify the Clausius hypothesis so greatly as materially to alter its character. It is no longer impossible to detect the free ions in a solution. Indeed, now, it is not only possible to detect them ; we can count them also. The nature of our new knowledge on this subject may be sufficiently illustrated by a single example, though many other phenomena might be quoted of no less importance than those I have selected.

Every one knows that in order to freeze salt water or brine it must be cooled to a lower temperature than is required in the case of pure water ; that sea water, for example, does not freeze so easily as river water. Now it has been discovered, first by Raoult, that in the case of dilute solutions of many soluble substances, such as sugar, this lowering of the freezing point of a solution is connected in a simple manner with the weight of the molecules of the dissolved substance, and in such a way that the latter can be deduced from the former when solutions of known strength are employed. But it appears that, whilst a large class of substances conform to this rule, there are notable exceptions. All the acids, bases, and salts—that is to say, the electrolytes, the very substances we have been considering—are exceptions. Solutions of these behave exactly as we might expect if some of their molecules were broken up into ions, as was suggested by Clausius. And the amount of the divergence often is so great as to indicate that not merely a few, but a considerable number of the molecules are thus broken up. Further, the number of molecules ionised in any given case is increased, as might be expected, by diluting the solution; the effect of diluting being, indeed, so great that some very dilute solutions contain comparatively little of the undissociated salt, but consist chiefly of free ions.

It would burden the reader if I put forward the whole mass, or even a fraction of the whole, of the evidence that exists in support of the ideas which have thus revolutionised the notions of many of us concerning the constitution of a class of compounds which until lately were considered to be typically stable, or permanent, in their character. Therefore it is only possible here to give a bird's-eye view of the various stages by which this new *révolution chimique* has been brought about, first, through the researches of the English electro-chemists in the Royal Institution, and, secondly, by the labours of Clausius, Arrhenius, van 't Hoff, Ostwald, and their numerous and eminent colleagues. But there is one other point which I must not omit altogether.

How do these ideas of the modern physical chemists adapt themselves to what we know about the relative activity of chemical compounds?

A very good idea of the position of the new theory in this regard can be formed from what has been learnt about the behaviour of the chief acids with the alkalis.

It used to be taught that sulphuric acid is a very strong acid, because it has the power of expelling so many of the other acids from their salts. But we now recognise that this power of sulphuric acid may depend more on the volatility of the acid expelled than on the strength of the attraction of sulphuric acid for the metals of the salts it decomposes. For we find that when the attractions of the acids for the alkalis are compared by methods which eliminate such complications as may be caused by differences in the physical properties of the substances studied, first, that sulphuric acid is really a weaker acid than some of those which it so readily drives from their combinations; and secondly, and this is most important to us, that the figures which express the "affinity constants," as they are called, of the acids express almost equally well their electric conductivities—that is, their state of ionisation. The agreement of these values is, in fact, so close, that in order to learn the relative affinities of two acids for an alkali, it is only necessary to ascertain their relative conducting powers. In other words, the chemical strength of an acid—its real chemical strength, that is, as measured when all interfering agencies have been eliminated—depends upon the number of ions present in its solution. Hydrochloric acid, which is highly ionised, is a stronger acid than sulphuric acid, which is less highly ionised; and this latter, in its turn, stronger than the still less ionised acetic acid, the acid of vinegar, which, again, is a stronger acid than carbonic acid, one of the least ionised of them all.

Here we seem to reach, at last, the common cause of the chemical activity of a chemical compound, and of its behaviour under the influence of an electric current. Both may be supposed to depend upon the tendency of the molecules of the compound to break up into ions when they enter into solution. Compounds in which the constituents are strongly bound together, like sugar and many organic substances, are chemically inactive and are bad conductors. Those which break up into their ions more freely are more ready to react and are better conductors, and the best conductors—that is, the most highly ionised substances—are chemically the most active of all.

This, then, it seems, is the origin of that identity of chemical and electrical phenomena which Davy was the first to foresee, and which was established by Faraday as one of the fundamental facts of physical science by his brilliant experimental researches during the first half of the nineteenth century.

And here chemical theory rests for the moment. But only for the moment. Sooner or later this, the newest theory of chemistry, like all its predecessors, will have done its work, and will give way to some newer and better tool. For it must be remembered that a scientific theory is not merely an explanation, but a tool: as much a tool as the hammer and saw of the carpenter, the scalpel of the surgeon, or the planing machine of the engineer. Like these everyday tools, it is intended to be used, and will be used, just as long as it is useful, but no longer. When its work is done it becomes, to borrow a phrase from the engineers, "scrap," and then the sooner it is replaced by a newer and better instrument the better it is for every one concerned in the matter. And yet, though it is true that every theory as a whole sooner or later becomes mere "scrap," the reader will detect an element of error in the statement. The parts of every great theory may be, and probably will be, used again. The original theory of Dalton has become, as a whole, insufficient as an explanation of chemical phenomena. The hypothesis of Clausius, after half a century, is found to be inconsistent with the real facts. Yet neither of these hypothesis is truly dead. We recognise the influence of each of them in the latest word of chemical science. No! Great scientific theories do not die. They do not go to the scrap-heap like worn-out machines. They are transformed, elevated step by step to higher planes. For is there not an element of truth in every one of them?

THE MECHANICS OF CHEMICAL CHANGE

In science, as in every other field of human endeavour, the centre of interest is for ever shifting. Yesterday the chemico-biological discoveries of Pasteur and his colleagues held us all enchained. The day before, Faraday's researches in electricity riveted attention. To-day we all watch the borderlands where chemists and physicists are busy unravelling the mystery of radium and radio-activity. But at all times underneath these waves of thought great currents flow quietly, steadily, and, by most of us, unseen, which are not less important than the more visible disturbances, however great these may be.

One of these great currents forms the subject of this essay.

I wonder if my readers have ever pondered on the subject of the great force which presides over those changes, called chemical changes, which play so tremendous a part in the world we live in? Most of them, no doubt, have often heard the term "chemical affinity." A few, perhaps, have a tolerably definite idea that by chemical affinity we mean the cause of chemical combining; the property of the chemical atoms which causes them, as we say, to unite and form all the thousands and thousands of combinations which we meet in nature and which chemists produce in their laboratories. Probably very few are aware of the fascinating researches and speculations on this subject which have occupied the minds of chemists more or less continuously for many centuries. And yet the results of these researches and speculations are hardly less interesting, and hardly less mysterious than that exciting substance radium and the other radio-active substances, though, as I have said, they are by no means equally familiar to most of us.

It would be absurd, here, to attempt to trace back the history of our subject to the thirteenth century, when Albertus Magnus, Bishop of Regensburg—who was said to be *magnus in magia naturali, major in philosophia, maximus in theologia*—an upholder of the birds-of-a-feather-flock-together theory of chemical action, employed the word "affinitas" to express the idea that substances which combine must have some kinship, something in common. Partly because this idea ignored the very obvious fact that the substances which combine most vigorously are just those which are most unlike one another, but still more because until Robert Boyle—"the father of chemistry and the uncle of the Earl of Cork"—had taught us in his famous book "The Sceptical Chymist" (1661) to discard the elements of Empedocles, earth, air, fire, water,[1] and enunciated the modern axiom that we must regard as the true "elements" those substances which have never yet been decomposed, no real investigation of the subject from the modern point of view was possible.

Boyle's book marks an epoch, but even after its publication the rate of progress was still so slow that when Liebig wrote his famous "Familiar Letters on Chemistry" half a century or so ago, he was able to dispose of the subject of "Chemical Affinity" in a few pages in one of the earlier letters. "In order," he says, "to obtain a clear and vivid comprehension of the almost miraculous order and regularity in which bodies enter into combination with each other, we must bear in mind the meaning the chemist attaches to the terms combination and decomposition," since all or nearly all the more familiar chemical phenomena depend upon these. And he went on to tell his generation that the ultimate causes of these chemical phenomena are "chemical forces" which "differ from all other forces, inasmuch as we perceive their existence only by their manifestations when bodies come into immediate contact with each other." Also that this chemical force or "chemical affinity" is strongest between elements which differ most in their general properties, and weakest in the case of elements which are members of the same or allied families. And, again, that the action of "chemical affinity" may be modified by means of heat, and by the presence of water or other solvents to such an extent that the chemist can use the different behaviour of substances in solution in different liquids, and their deportment at high temperatures as a powerful means of analysis in the laboratory.

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[1] Usually attributed to Aristotle.

To-day this matter seems by no means so simple. On the contrary, it positively bristles with unsolved questions which are at once, if I may be excused the paradox, sources of obscurity and of enlightenment.

But the truth is that even in Liebig's time the question of the nature of chemical affinity and its mode of action was not so simple and well understood as we might suppose from some of the writings of that period. Chemists were too busily occupied just then in laying the foundations of organic chemistry and physiological chemistry, and, generally, in extending the boundaries of their science and occupying new and recently acquired fields of research, to have much time to study affinity.

Nothing is much more difficult than to state quite clearly the relative affinities of half-a-dozen common elements. We can compare the masses of the atoms of these elements, though millions of millions of millions of them will go into a lady's thimble. We can compare their powers of conducting heat and electricity, find their melting-points, and learn a dozen other things about them. But when we attempt to decide whether an atom of an element A or an atom of an element B has the greater attraction for an atom of a third element C, we find the problem almost as perplexing as to tell beforehand which of two maids a man will fall in love with. It has been done, it is true, within certain limits, but it is most difficult, the complications are so many. Let me give a few illustrations :—

Suppose we mix oxygen from the air with the gas hydrogen in a glass tube and leave them together for a few hours, days, or weeks. Nothing will happen; they will remain a mixture of oxygen and hydrogen. Oxygen and hydrogen, therefore, we might conclude, are without chemical attraction. Suppose we heat this mixture, however, at any point by means of an electric spark, or a very hot wire. When we do this we find that the gases combine, and if they are in suitable proportions nothing but water remains. Oxygen and hydrogen, then, acquire the power of attracting one another when heated. It is all a question of temperature. They attract each other when hot, not when cold. High temperature, in other words, promotes the action of chemical affinity. But suppose we go a step further. Suppose we take the water formed at the second stage of our experiment, convert it into steam, pass a torrent of electric discharges from an induction coil through it, and examine what remains. If we do this we shall find, in the expressive language of a youthful pupil of mine, that we have "electrocuted" some of the water, that we have reconverted part of it into oxygen and hydrogen. What can we say now about the mutual affinity of oxygen and hydrogen? See how entirely their power to attract one another seems to depend upon temperature, how they rival "Mr. Facing Bothways" in their disposition to accommodate themselves to circumstances.

Again, there is a very simple method of making iron, the metal, from iron rust, which is a compound of iron and oxygen. All we have to do is to pass a current of hydrogen over the iron rust heated to redness in a tube. Then water is formed from the hydrogen and oxygen, and metallic iron remains as a black powder which has the peculiarity of catching fire and burning up like so much charcoal if it is exposed to the air when fresh. May we not safely conclude from this that, since hydrogen can withdraw oxygen from iron rust, the affinity between hydrogen and oxygen must be greater than that between iron and oxygen? Certainly, we should be tempted to think so if it were not quite easy to get evidence in support of exactly the opposite conclusion: if it were not for the fact that if one passes steam—oxide of hydrogen—over red-hot metallic iron, instead of the element hydrogen over oxide of iron, then the oxygen leaves the hydrogen and unites with the iron, forming an oxide, which I may say is magnetic like the loadstone, while the hydrogen goes free. Now, would not this, taken by itself, indicate quite definitely that iron, and not hydrogen, has the greater attraction or affinity for oxygen ?

What are we to think on this subject ? What is the explanation of these and similar contradictory phenomena ?

Chemists have long been busy with this question. It was brought to the front by Berthollet in an historic discussion with Proust, his fellow-countryman, on the subject of the law of definite proportions, which expresses what we regard as the established truth, since no well-established fact to the contrary can be produced, that every given chemical compound, for example water, always contains the same elements, and always in the same proportions.

According to Berthollet, the elements may be considered to possess affinity or attraction for one another, and this affinity is one of the determining causes in any given case of chemical change. But it is not the only determining cause ; its action is subject to the influence of various physical forces, and to the influence of the masses of the acting substances. Berthollet's famous memoir was produced under most interesting circumstances. When Napoleon the Great undertook his expeditions to Italy and Egypt he did not go only with armies of soldiers, he took with him, also, companies of the leading French *savants*. Among the foremost of these *savants* was Claude Louis Berthollet, and it was at a sitting of the "Egyptian Institute" at Cairo in July 1799 that Berthollet read the famous memoir on the Laws of Affinity, in which he asserted that the action of affinity and the progress of chemical changes are largely dependent on the solubility, insolubility, volatility, &c., of the substances concerned. Berthollet did not convince his colleagues that only insoluble substances, volatile substances, &c., contain their constituents in fixed proportions, for the facts were against him. But he advanced in this memoir, of such romantic origin, and a few years later in his great work on chemical statics, ideas which play a leading part in chemistry to this day.

I have already discussed one aspect of the new chemistry in the preceding essay. And there I showed how the results of modern research force us to admit that the chemical atoms in acids, bases, and salts are by no means so rigidly held together in their molecules by chemical attraction as was formerly supposed; how we now believe with Faraday that chemical actions are indistinguishable from electrical actions; and with

Arrhenius and his colleagues that these apparently stable substances when in solution, and often, also, when fused by heat alone, are to a greater or less extent in a decomposed or dissociated state ; so that a solution of common table salt, for example, in water consists only partly of salt, and partly, or if the solution be a very weak one largely, of its *ions* (*i.e.* of atoms of sodium and atoms of chlorine bearing respectively charges of positive and negative electricity) wandering among the molecules of salt and water. Nor does the change in our ideas stop here. For we conclude further, that such a solution, though it seems so quiet, is really a scene of intense activity ; that, in it, salt molecules every moment are breaking up into their *ions* by thousands, and that these *ions*, the positive and negative particles, are, as the result of their frequent encounters as they wander in the liquid, constantly reuniting to form salt. We have substituted, in short, dynamica conceptions for the statical conceptions which held the field a century ago. We see, or believe we see, a state of change where our predecessors saw a state of rest.

If we pursue this new conception of the constitution of a solution of an electrolyte, as we term the class of substances we now are considering, we shall perceive that the ideas of our predecessors were not so much wrong as imperfect. For is it not plain that in a state of affairs such as I have pictured, things would arrive quickly if not at a state of rest at least at a state of equilibrium, which easily might be mistaken for a state of rest? For we see, on consideration, that when common salt is dissolved, or melted, its molecules must, according to the hypothesis before us, begin to break up one by one into their *ions*, into sodium ions and chlorine ions, and that for a moment this change alone will occur. As soon, however, as there are some positive and negative *ions* wandering in the solution, these will meet from time to time, and when they meet probably will reproduce molecules of the original salt. At first the number of the free *ions* will be very small in proportion to the number of molecules of the original salt. But as the salt molecules continue to break up the number of these will diminish, while the number of free *ions* will correspondingly increase. Thus the rate at which the salt molecules break up must steadily become slower from the first, while the rate at which the free ions recombine and reproduce salt will steadily become more rapid, until at length salt will be produced by the latter change as quickly as it is destroyed by the former change, and so a state of balance or equilibrium will be reached in which the proportions of salt and of its positive and negative ions present in a given volume of the solution will remain unchanged. Ideas of this sort play a great part in modern chemistry, and may be said to form the foundation of what we term "Chemical Dynamics." In this branch of chemistry we concern ourselves with chemical changes, and endeavour to trace the movements and transpositions of the atoms in those changes. In chemical dynamics we do not deal with single substances, with views about the atomic constitution of matter, or about the arrangement of the atoms in the molecules—these belong to "Chemical Statics"—but devote ourselves to the mutual actions of groups of substances in chemical changes, to the rates or velocities at which such changes progress, and to chemical equilibrium. Only a few years ago "chemical dynamics" occupied a comparatively minor place in the science. But to-day the most advanced masters claim for it the first place. It will be impossible, of course, to do more here than give in outline a few illustrations of this facet of the New Chemistry.

And now I must ask my reader to go back with me for a moment to the beginning, so to speak, of our subject. This is necessary if we are to gain a clear comprehension of the present state of knowledge on the subject before us. Chemists assume, at any rate as a working hypothesis, that in chemical changes they have to deal with very small particles of matter. We call these particles *atoms,* and hold that every element has its characteristic atom. Further, we assume that the atoms of the seventy or eighty elements known to us come together in various groupings, and so form the thousands and thousands of decomposable substances, called *chemical compounds,* met with in nature or produced in the laboratory.

Now the weights of the atoms are believed, on very good grounds, to be fixed and definite for each element, and, therefore, we can represent them by symbols. Thus the wonderful phosphorescent element phosphorus has atoms which weigh thirty-one times as much as an atom of hydrogen, and are said to have the atomic weight 31. This atom is represented in the algebra of the chemist by the symbol P. Similarly atoms of chlorine, the stinking gas used for bleaching calico and other things, also have a fixed weight, 35.5, and are represented by the symbol Cl.

Suppose, now, we wish to represent the product of combining chlorine with phosphorus, we may write it PCl_3 or PCl_5, for there are two such compounds, and the proportions of chlorine and phosphorus in them are in the first case 106.5, *i.e.* 35.5 multiplied by three, and in the second 177.5, *i.e.* 35.5 multiplied by five, of the former to thirty-one parts of the latter, which makes it plain that one atom of phosphorus can combine with three or with five atoms of chlorine, according to the quantity presented to it, and, as we shall see shortly, to other circumstances. I must add that the above formulæ represent what chemists call *molecules* of the two chlorides of phosphorus, a molecule being the smallest particle of any given substance which we can imagine as existing alone. It is clear that a molecule could not be less than the quantities respectively represented by the formulæ PCl_3 and PCl_5, for in order to get smaller molecules we should have to cut up the atoms of phosphorus, and, as I have said, atoms are *ex hypothesi*, particles which cannot be subdivided.

I have selected these examples of the symbols and formulæ of the chemist because with their aid I can give the reader a clear idea of what we mean by a *reversible chemical change*. If some of the second or pentachloride of phosphorus,

PCl_5, is heated in a closed vessel, we find that its vapour changes into a mixture of the trichloride, PCl_3, and free chlorine, Cl_2, which fact, thrown into a chemical equation, is expressed thus:

$$PCl_5 = PCl_3 + Cl_2.$$

If we then cool the vessel the pentachloride will be re-formed, which, as an equation, is written:

$$PCl_3 + Cl_2 = PCl_5.$$

That is to say, that while the pentachloride of phosphorus is decomposed by heat, its components reunite on cooling, provided that the products of the first change are kept together.

We call a change which can thus take place backwards and forwards a reversible change, and in books on chemistry such a change is written:

$$PCl_5 \rightleftarrows PCl_3 + Cl_2$$

the two arrowheads being pointed in opposite directions to indicate the fact that the pentachloride, PCl_5, splits up, or the products of its decomposition recombine, according to circumstances. It is possible, in fact certain, that these two opposite changes may proceed simultaneously.

All chemical changes are not so obviously reversible as that discussed in the previous paragraphs, but very many are so, and those which are not often owe this to the circumstance that the conditions under which they occur are such as to prevent the second or reverse actions from coming off. For example, we all know that for thousands of years quicklime has been made by heating limestone, and that kilns for making lime in that way are common objects on the countryside. Now, if limestone be heated in a practically unlimited space, as in the open air, or in a powerful draught of air, as in a limekiln, it breaks up into quicklime and a gas called carbonic acid gas which rapidly flies out of reach of the quicklime never to return, and so presently the latter only is left. This change carried out in this way seems not to be reversible.

But if a piece of limestone is heated in a strong, exhausted, air-tight vessel, so that the gas cannot get away from the lime, but must stay in contact with it, and if the vessel is provided with a pressure gauge, so that we can measure the rate at which the gas is given off by its effect on the gauge, we shall discover that the limestone is not decomposed suddenly as in an explosion, or at one particular temperature, but gradually, and at an increasing rate as we increase the temperature to which we subject it.

Now this at first sight is rather surprising.

How can such a thing be? The gas begins to come off somewhat below 600° C. That is to say, some particles of limestone are decomposed at 600° C., and, on the other hand, other particles of the same substance remain unaffected although they are heated equally strongly. This seems absurd. Surely all the particles of the same substance, quicklime, would be decomposed at 600° C. if any are so decomposed. At any rate we think this would be so if we maintained that temperature for a sufficiently extended period.

But suppose the change is reversible. Suppose that as soon as some particles of limestone are split into quicklime and carbonic acid gas the latter substances begin to recombine, reproducing limestone. Then, is it not likely that these two opposite changes would tend to balance one another?

There is a simple way of testing the validity of this hypothesis, viz., to try to perform the experiment backwards and observe whether the results obtained in that way are consistent with it. Now suppose we heat some pure, dry limestone in our apparatus to certain fixed temperatures; for example, to 600° C., to 650° C., and to 700° C. We shall find that a certain pressure called the "dissociation pressure" can be reached at each temperature, but not a greater pressure.

At 700° C., for example, this maximum pressure will be equal to that of a column of quicksilver about 15 centimetres high. We shall find, further, that this pressure does not depend on the amount of limestone taken, nor on the amount of quicklime produced by decomposing the limestone, but only on the temperature applied. So that if at 700° C., when the pressure has reached the maximum, we pump away the gas till we have reduced the pressure to the zero point and then cease pumping, but maintain the temperature at 700° C., the pressure will rise again gradually to the same maximum as before. And so again and again, although the amount of quicklime is increasing and the amount of limestone decreasing, until all the limestone is destroyed and nothing but quicklime remains in the tube. If next, when nothing but quicklime is left in the vessel, we allow carbonic acid gas to flow into it till we have filled it to a pressure equal to that of the barometer, say 760 millimetres, keeping it as before at 700° C., we shall find that the gas will be absorbed and the pressure will fall till it reaches its former maximum of 15 centimetres for this temperature; and if afterwards we lower the temperature, first to 650° C. and then to 600° C., the pressure will fall still lower, viz., to the maxima corresponding respectively to 650° C. and to 600° C. Finally, if at 600° C. we again raise the pressure by adding more carbonic acid gas, it will gradually fall back to the pressure corresponding to 600° C., and will do this again and again until the quicklime is reconverted into limestone once more.

There is, therefore, no doubt that, as the facts quoted made us suspect, there are two changes, viz., one by which limestone is destroyed and another by which it is reproduced, that these two changes both of them go on at the same time, in the same tube, at the same temperature; and that the state of equilibrium, which seemed so puzzling at first, was due to the fact that these two opposite processes proceeded simultaneously at equal rates, and thus exactly balanced one another.

Increase of temperature, as we have seen, causes a rise in the "dissociation pressure" of quicklime, because more limestone is decomposed, and more carbonic acid gas given off. Now, why has increase of temperature this effect? Do not the facts seem to imply that there are differences in the rates, or velocities, at which limestone splits up at different temperatures, and differences perhaps in the rates at which quicklime and carbonic acid gas recombine? And is it not possible that these facts might give us a means of comparing or measuring these velocities?

We can now understand why limestone behaves so differently when heated to about 600° C. or 700° C. in a limekiln through which a current of air is passing, and in a tightly closed vessel. In the former case the affinities of the quicklime and carbonic acid gas have but little opportunity of coming into play. When limestone is heated in a limekiln to a red heat decomposition ensues, as in a closed vessel; this may proceed until the dissociation pressure corresponding to the temperature of the furnace is reached, and if the top of the kiln were closed the action would then stop, as it does in a closed vessel, owing to the recombining of the products of the decomposition being in equilibrium with the opposite process. But the current of gases which passes through the kiln from the bottom to the top sweeps away the carbonic acid gas almost as fast as it is liberated. Thus it has very little opportunity to reunite with the quicklime, and the reverse action does not take place, but only that in which the limestone is destroyed, and so the latter is quickly and completely "burnt" into lime. There is a very elegant experiment illustrating this point. It consists in heating two crystals of limestone in the form of Iceland spar, equally, side by side in two tubes, and passing air through one tube and carbonic acid gas through the other. When this is done at a suitable temperature the crystal heated in air quickly loses its lustre and becomes opaque, as it gives up carbonic acid gas and is converted into quicklime, while the other crystal, owing to the presence of carbonic acid gas, is scarcely affected.

If, now, we apply what we have learnt from the behaviour of limestone to the contradictory behaviour of steam and iron, and of iron oxide and hydrogen, quoted on a previous page, we see a possible explanation of the mystery. The fact is this. When the chemist passes steam over iron filings in order to make hydrogen he uses practically unlimited quantities of steam, and so sweeps away the hydrogen as quickly as it is formed. Hence the hydrogen has little opportunity of reacting with the oxide of iron produced in the direct change:

Steam + iron = iron oxide + hydrogen,

and so in time all the iron is converted into oxide of iron.

On the other hand, when the chemist reverses the process and passes hydrogen over iron oxide, he uses unlimited, or, at any rate, very large quantities of hydrogen, and arranges his apparatus so that the steam produced in the change:

Hydrogen + iron oxide = steam + iron

is swept very rapidly away from the iron, and given little or no opportunity of reversing the change by reacting again with the iron. In other words, in each of these experiments he prevents the establishment of a condition of equilibrium among the substances concerned by sweeping one of the products of the change out of the field of action. Thus the results of these experiments throw no light on the relative affinities of iron and hydrogen for oxygen; though they illustrate admirably the fact that the progress of a chemical change does not depend only on the affinities of the substances concerned in the change.

It would be easy to multiply examples of this kind, and to show how similar effects can be brought about in the case of substances which by their action upon each other in solutions produce solid precipitates, or gases which escape as they are formed, and so determine the final result. But to go into greater detail probably would only confuse, and enough has been said to show how essentially dynamical are the conceptions of modern chemistry. Though, for convenience, we still combine our symbols in equations which suggest that the molecules after a reaction are in a state of statical equilibrium, their atoms at rest, held together by the attraction called "chemical affinity" ; yet, when we contemplate the behaviour of these molecules in relation to one another, we frequently find it necessary to admit that such a state of rest is quite inconsistent with the facts of the case, and are driven to conclude that the atoms which compose the molecules are really in a state of continual motion, that no single molecule lasts very long, that all from time to time break up into their constituents, or enter into reactions with other molecules which may approach them ; and that the products of these changes, equally unstable, reverse these effects at an equal rate, a slower rate, or a faster rate, according to the conditions which may prevail for the moment.

There is one other important department of our subject which we must not pass by. If you prepare a strong hot solution of the salt known to photographers as "Hypo" in water, and set it aside in a flask well protected from air, and from the dust which air always carries, your solution as it cools will remain clear and transparent almost indefinitely, or at any rate for a very long time. But if you expose it to the air, and especially if you touch it with a fragment of the original salt, even though the fragment be microscopically minute, a change will instantly set in. Crystals will start from the nucleus and spread rapidly, so that in a few seconds the whole contents of the flask, however large it may be, will be transformed into a solid mass of crystals. Such a solution, before it comes into contact with the solid salt, is known as a super-saturated solution. Its parts are in a state of equilibrium. Yet so unstable is this equilibrium, as you can easily prove for the experiment is one which any one can perform, that mere contact with an almost unweighable portion of the solid salt determines changes of a relatively tremendous character. The action of the crystal in this experiment may be regarded as physical. But there are plenty of analogous examples of "contact action" to be met with in chemistry in which it is found that the presence of a definite substance accelerates and sometimes starts a given change without the stimulating substance itself being sensibly altered in the process. Finely divided platinum, for example, by its mere presence, promotes the breaking up of hydrogen iodide into its constituents, hydrogen and iodine; and also, it is found, promotes the reverse process, the combining of hydrogen with iodine to form hydrogen iodide. A number of interesting changes of this class are discussed in the essay on "Ferments." But one case, the influence of traces of water, is so remarkable that it deserves mention here.

In quite an astonishing number of cases chemical change has been found to take place only in the presence of a trace of water vapour. Thus, highly combustible substances such as charcoal, sulphur, and even phosphorus may be heated quite strongly in air or oxygen and yet not burn, if both have been thoroughly dried beforehand. And again, while ammonia, the gas given off by smelling salts, will readily combine with hydrochloric acid — the spirit of salts of the Middle Ages—when they are cold, and while the product of their combination, ammonium chloride, is readily broken up by heat, neither of these changes will take place if the materials employed are thoroughly dried before the experiment is made. So many cases of this kind have been discovered that at one time it was beginning to be suspected that chemical change might be impossible *in the absence of water.* The facts as a whole do not, however, at present support this idea, nor does it recommend itself to most of us as inherently probable. Nevertheless, the phenomena of contact actions are of the greatest possible interest and importance. Various explanations have been put forward, but it cannot be said that we have a satisfactory theory of the subject. But the fact remains, and it is one of the most interesting in all chemistry, that certain substances, and among them water, can by their mere presence, as it were, increase the velocity of chemical change in certain cases to such an extent that processes which usually proceed so slowly that they seem to us not to occur at all, may proceed with explosive rapidity in the presence of a mere trace of a catalyst. The mystery of radium is hardly more obscure than these contact actions. Unfortunately the subject is, experimentally, a very difficult one, months or even years sometimes being consumed in the necessary preparations for a single simple experiment, and, worst of all, the issue has been confused too often by impatient investigators working upon impure materials in a field where purity is everything.

RADIUM

THE twentieth century had scarcely started on its course, when the educated world became aware that France already had done something calculated to make its opening years memorable, by presenting to us a new element as unique in its properties as phosphorus must have seemed to King Charles II. in 1677, or the metals sodium and potassium to Sir Humphry Davy's audiences at his lectures at the Royal Institution a hundred and thirty years later. Science is of no country, and our colleagues across the Channel sometimes have been criticised pretty sharply for claiming chemistry as a "French science," in virtue of the work of Lavoisier. But we must all concede that radium is indeed a French element, for not only was it detected and isolated by two French chemists, Madame Curie and M. P. Curie, but their discovery was the direct consequence of the previous recognition of the Becquerel rays, and the property of matter known as radio-activity by another eminent Frenchman, M. H. Becquerel.

The story of radium and the radio-activity of matter probably has not yet run beyond its earliest chapter—has, indeed, scarcely got beyond its prologue. But already, as we shall see, this great discovery opens out to us new paths and a new horizon, new paths which ten years ago we did not even imagine to exist; and a new horizon which, as yet, is shadowy and almost beyond our range of vision, but which clearly, as we feel, offers illimitable fields for exploration to those who are able to press on towards them. French contributions to science have always been illuminating in a high degree. This latest gift assures us that French science remains in the twentieth century, as ever, a star of the first order of magnitude.

In the pages which follow it will be necessary to use the ideas and language of various current hypotheses; and as the truth of some of these hypotheses may be open to question, as they are working hypotheses in fact, I may be excused if I remind my readers in advance that "our journey is not to these, but through these," that they are but tools forged for a definite purpose, and will be cast aside as soon as better ones are within our reach. We must not judge M. Becquerel, Madame Curie, M. P. Curie, Prof. Rutherford, and their brilliant fellow-workers by the tools they have employed, but by the work they have done with them. It may be that none of these hypotheses will ultimately prevail, that much more commonplace explanations will replace them. That does not matter, radium and radio-activity are not only new, but also true, and we may be sure that the study of these new, true, and wonderful things will lead us, sooner or later, to recognise other truths equally important, and equally unsuspected and astonishing.

Up to the present the amount of radium that has been obtained in the state of a pure salt is very small. When Madame Curie determined its atomic weight, all that was available for her work was about one and a half grains of the chloride; and to get this it had been necessary, she tells us, to work up the greater part of the stock of impure radium then in her possession.[1] But with this small quantity Madame Curie was able to determine that atoms of radium are about 225 times as heavy as those of hydrogen, and to discover that radium, in its general chemical character, seems to belong to the same group of elements as calcium, the metal present in the builder's lime which we make in vast quantities by roasting chalk and other limestones.

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[1] A good deal more exists now. Pure radium salts have become articles of commerce; they can be purchased by the milligram, but the total amount in existence is still a mere matter of a few grams.

But though the salts of radium and those of the allied metals are much alike in many respects, the difference between them is in reality prodigious. In the first place, radium salts are self-luminous or visible in the dark, like phosphorus, though from a very different cause. Secondly, when a radium salt is brought near a cardboard screen coated on one side with the platino-cyanide of barium, the platino-cyanide glows with a green light as long as it is under the influence of the radium, but no longer. Thirdly, radium salts impart a remarkable phosphorescence to the preparation known as Sidot's hexagonal blende, sulphide of zinc, and in this case the effect persists for a little while after the salt has been removed. Sir William Crookes gave an interesting account of this last quality of radium nitrate in a paper read before the Royal Society. He tells us that glass vessels which have contained radium salts become radio-active, and remain so in a most persistent manner, so that even after being washed they will cause a screen coated with hexagonal blende to glow as it does in the presence of radium itself, and that diamonds brought into the neighbourhood of radium nitrate glow with a pale greenish light, just as they do under the influence of the kathodic bombardment in a radiant matter tube. If minute particles of the radium salt come into contact with the blende screen, its surface is at once dotted with brilliant specks of light about the size of small mustard seed, even though the particles themselves are too small to be detected in daylight. Under a lens magnifying about twenty diameters each of these specks of light is seen to consist of a dull centre surrounded by a halo; from the centre of each speck light shoots out at intervals in every direction, and the surface of the screen around the halo is bright with scintillations. If a piece of radium nitrate is brought very near the screen, the scintillations are so close together that the surface of the latter, when examined through a lens, looks, as Sir William Crookes expresses it, like "a turbulent luminous sea"; but if the distance between the radium and the screen be made greater the scintillations are fewer, and the effect is that of stars on a black sky. Finally, if the salt touch the screen, the spot touched remains bright with scintillations for weeks afterwards.

Thin sheets of glass or of aluminium placed between the screen and the radium salt stop the scintillations, but do not destroy the power of the salt to produce phosphorescence. When the cards carrying the chemicals are placed face upwards above a radium salt, so that the radiation from the latter must pass through the card to reach the sensitive surface, the platino-cyanide screen still becomes luminous under the influence of the radiation, but the blende screen shows no scintillations. Thus it seems clear that the radiation which causes scintillations cannot pass through card. But since this radiation must be arrested also by the card bearing the platino-cyanide of barium, and since the platino-cyanide is nevertheless rendered luminous, it would appear that we have to deal with at least two distinct radiations from radium, viz. one which produces the scintillations detected by means of Sidot's hexagonal blende and cannot pass through cardboard, and a second which does not produce scintillations but can pass through cardboard.

When radium salts, or mixtures rich in radium salts, are brought near the closed eyes or to the temples, a peculiar sensation of light is perceived, not only by those who possess efficient eyes but even in some cases, it is said, by the blind, a fact which, if true, may explain some of the vague accounts of new remedies for blindness which appeared two or three years ago in the newspapers. But those who may have opportunities for handling radium, or any other strongly radio-active substance, will be wise to be very careful in making experiments involving the eyes, for exposure of the tissues to radium rays is apt to be followed by redness and irritation, and finally by ulcers which are slow to heal. It should be added that actual contact with the skin is not required to produce these unpleasant effects, for on one occasion an observer who had carried a few grains of a barium salt strongly impregnated with radium in his waistcoat pocket for a while was troubled afterwards by a sore which took nearly a month to heal, although the radio-active matter had been packed in a glass tube in a cardboard box wrapped in paper, and was further separated from his body by at least two layers of cloth.

A most wonderful property of radium, first made known to us by the late M. P. Curie and M. A. Laborde, is its power of generating heat. Speaking generally, we may say that every body on the earth's surface tends to assume the same temperature as its surroundings. If it be hotter than the other bodies near it, then it will give to the latter more heat than it gains from them, and will gradually cool till all are at the same temperature.

If it be colder than its surroundings, on the other hand, then it will gain heat more quickly than it loses heat, and so will rise to the temperature of the other bodies in or about its neighbourhood. The most unscientific of us, in effect, acts upon the assumption that this is the case when he uses a thermometer to take the temperature of a cellar, or when, feeling ill, he takes his own temperature. Only when there is some source of supply to compensate a body for the heat lost by radiation, as, for instance, in the case of a living man or animal, in whom heat is generated by the oxidation of the food, or in ordinary cases of chemical change, as in a fire, or when heat is supplied to a body by electrical or other means, can its temperature be maintained for long above that of its environment. But radium seems, at first sight, to be an exception to this general experience, for M. P. Curie and M. A. Laborde, by placing one gram (15½ grains) of a sample of radiferous barium chloride, containing about one-sixth of its weight of radium chloride, in a small bulb together with a thermo-electric couple (a kind of thermometer), found that radium by no means takes the temperature of the surrounding air, but, on the contrary, remains steadily about one and a half degrees centigrade hotter than this. From this, and from the results of other experiments, they calculated that the two grains or so of radium chloride used in their experiment gave off enough heat every hour to raise the temperature of its own weight of water from 0° C. to 84° C., a temperature which is not so very far from the boiling-point, 100 degrees, of the latter substance; whilst, from the results of other experiments in which a nearly pure radium salt was used, they calculated that in each hour the amount of heat evolved by an atomic proportion of radium (225 parts?) is not so very much less than that produced by burning an atomic proportion of hydrogen.[1]

Broadly speaking, these results have been confirmed by the work of other observers. Their surprising character will be better understood when I point out that the oxyhydrogen flame is one of our most intense sources of heat, and that when, in an oxyhydrogen flame an atomic proportion of hydrogen has once evolved its 34,000 units of heat it is changed, with the oxygen consumed, into water, and is then incapable of yielding further similar effects, but that radium, on the other hand, is not, it would seem, thus limited in its powers. This can evolve, practically speaking, as much heat in a second hour as in the first, as much in a third as in the second, and so on for a very long period, so that a single portion of 225 grams (about half a pound) of radium would give out heat enough in a single year to raise nearly two tons of water from the freezing-point to the boiling-point. Moreover, there is good reason to believe that its power would not be reduced in any perceptible degree at the end of one year or of several years. But we must not go too fast. The radio-activity of radium is not inexhaustible, and its power of generating heat would be very considerably reduced in such a period as a thousand years, for it is not by any means immortal. We will now pass on to the beautiful researches which led to the discovery of radium and the other radio-active substances.

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[1] The numbers were, for the radium 22,500 units, for the hydrogen 34,000 units.

Probably most of my readers at some time or other have seen chemically-prepared screens highly illuminated under the influence of the Röntgen or X-rays; and all will remember examples of the photographic or radiographic silhouettes taken by their aid for surgical purposes. The discovery in 1895 of the Röntgen rays, and of their action on fluorescent and phosphorescent screens and on photographic plates naturally suggested that further experiments should be made in the same direction. The chief result of these experiments was the discovery of uranium or Becquerel rays by M. H. Becquerel early in 1896. Becquerel's first recorded experiment was as follows: Having wrapped a photographic plate in two layers of stout black paper, and assured himself that it could be exposed to the sun for a day and yet remain unaffected, he placed upon the upper surface of the paper some crystals of a salt of the metal uranium, and exposed the whole for some hours to sunlight. Afterwards he developed the plate in a dark room in the manner familiar to photographers, and found he had a black silhouette of the crystals, or, in other words, that something photographically active, like light, but able to penetrate layers of black paper which were quite opaque to light, had been given out by the salt. When he placed thick metallic screens between the layer of salt and the photographic plate, their shadows appeared on the plate, showing that the new radiation was less able to pass through these than through black paper. But in subsequent experiments, M. Becquerel found that aluminium in thin sheets was transparent to the new rays like paper, and that even copper in very thin layers was partially transparent to the "uranium rays."

The salt which M. Becquerel employed, a double sulphate of uranium and potassium, becomes self-luminous when it is exposed to sunlight, though the effect only persists for the one-hundredth part of a second after the light is removed, and he tells us that at first he supposed the visible radiations which reached the photographic plate were due to, or in some way connected with, this visible phosphorescence. That is why he made his experiments in sunlight. But it happened one day that having prepared his apparatus for the experiment described above, he found the conditions unsuitable, owing to clouds, and put away his plates as they were, with the salt in position, anticipating a failure. But when he developed the plates a few days later, he obtained, not faint silhouettes, as he had expected, but particularly dark ones; and thus discovered, by accident partly, that he need not stimulate the activity of the salt by exposure to strong light. This discovery was more than amply confirmed by subsequent experience, both in the case of the salt used by Becquerel and of other uranium compounds, including some which, though radio-active like the rest, are not known to be fluorescent nor phosphorescent.

From the day when this happy accident occurred, rapid progress was made by Becquerel and others, and "radio-activity" was quickly recognised as a phenomenon of the first importance. Becquerel found the new rays were given off by every uranium salt he examined, and that metallic uranium was more active than any of its salts. Therefore he supposed the radiations owed their origin to uranium ; and he called them "uranium rays."

The experiment by which Becquerel obtained evidence that radiation can go on with but little aid from direct sunlight induced him to investigate the behaviour of the salt in complete darkness, in order that he might learn how long it would retain its power. This led to the discovery, already mentioned, that though exposure to the sun slightly increases the activity of the salt, yet if it is kept in the dark for weeks, months, and, as he found later, even for years, it continues to give off the rays, and, moreover, that their original intensity was maintained almost unimpaired, though no source could be assigned to the energy thus radiated day by day and year by year for apparently indefinite periods.

In the course of further investigations it was discovered that the "Becquerel rays" resemble the Röntgen or X-rays in many respects. Thus the former, like the latter, make air conduct electricity to such an extent that if a piece of uranium or one of its salts be brought near an electrified body surrounded by air, the charge gradually leaks away till all is gone, and small electric currents may actually be passed along a cut wire if a portion of a radio-active substance be brought near the gap where the cut occurs. Again, the Becquerel rays, like the X-rays, will cause air free from dust, but supersaturated with moisture to deposit part of its water in the form of fog, much as particles of dust are known to do. Also it was found that water, many solutions of metallic salts, paraffin, quartz, Iceland spar, and sulphur all are more or less transparent to these rays, that certain well-known red and blue glasses, and the yellow uranium glass so often to be seen in the windows of opticians are less so; whilst copper is not much less transparent than aluminium, but platinum somewhat more absorbent. As a general rule it was found that the Becquerel rays pass through the substances mentioned more freely than the X-rays, and that their behaviour, when transmitted simultaneously through several screens of different materials, varies in such a way, according to the order in which the screens are placed, as to lead to the conclusion that Becquerel rays, like X-rays, are not all alike, but consist of mixtures of more or less dissimilar radiations. M. Becquerel thought at first that the Becquerel rays could be reflected and refracted like common light; but later experiments failed to confirm this, and at present physicists find themselves unable to reflect, refract, or polarise these radiations.

With the year 1898, only about two years after the first of Becquerel's epoch-making announcements, a new chapter was opened in the history of chemistry. Just as the discovery of the galvanic pile by Volta in 1800 was promptly followed first by the decomposition of water by Nicholson and Carlisle, and secondly by the decomposition of potash and soda by Sir Humphry Davy; and just as the invention of the spectroscope was followed quickly by the discovery of many rare and interesting elements, including cæsium and rubidium, by its means, so the discovery of radio-activity by Becquerel soon led to chemical developments of a most interesting character.

It need hardly be said that the memoirs of Becquerel on the strange properties of uranium and its compounds caused other workers to be on the look-out for substances which might exhibit similar qualities. Among these were Madame Sklodowska-Curie and M. Schmidt, who found, independently, that oxide of thorium gives off radiations even more active than those of uranium. Then Madame Curie made a really great discovery. Noticing that some pitchblende, which is one of the sources of uranium, was more radio-active than might have been expected from the proportion of uranium present in it, and, in fact, more radio-active than uranium itself, this talented lady saw she was on the track of a new radio-active substance, and, jointly with M. P. Curie and M. Bémont, proceeded to separate samples of pitchblende into its components by chemical analysis, studying the radio-activity of each fraction in order to track to its source the cause of the great activity of the mineral. This mode of procedure was soon productive, and they quickly discovered, first, a substance, called polonium after Madame Curie's native country, which gave radiations one hundred times as energetic as those of uranium; and then, when examining the salts of barium obtained from the same mineral, they found that these were associated with traces of another extremely active element, so similar to barium in its general reactions that but for its radio-activity its existence might never have been suspected. This was radium.

The investigation of polonium proved difficult from the first; but the further study of the active barium salt gave more decisive results, as has already been indicated, though even in this case success came only by degrees. Working with the barium salt extracted from about two hundredweight of pitchblende, our investigators obtained a minute quantity, much less than a grain, of a preparation which, though still far from pure, was nine hundred times as active as uranium in imparting conductivity to the air. Next using still larger quantities of the raw material, they got larger and purer specimens, five thousand times, and fifty thousand times, as active as the original salt; and then, finally, Madame Curie, by repeatedly repurifying nearly all the radiferous barium chloride at her disposal, obtained a specimen of radium chloride far more active than any of these which was sufficiently pure to enable her to determine the atomic weight of radium, although the quantity of the salt thus made only amounted to about one and a half grains.

The new salts thus obtained exhibit a characteristic spectrum, and from their properties these salts seem to belong to the same group as the salts of metal calcium whose compounds are familiar to us in the limestone rocks, in alabaster, in plaster of Paris, and in various other forms. The atomic weight found by Madame Curie (225) may be low.[1] It is very difficult to obtain radium perfectly free from barium, and the presence of the latter would tend to reduce the apparent atomic weight of a specimen in which it was present. Before we pass from this part of our subject I may add that M. A. Debierne isolated yet a third highly radio-active substance from pitchblende. This is called Actinium. Its radiations are more like those of radium than those of polonium, but it is not self-luminous.

And now, it will be asked, what about the uranium which gave its name to the new rays? Is uranium not radio-active after all? Has it no powers of its own? Does it owe its activity entirely to companion substances, and is its part in this affair played to the finish? These questions at one time were not a little difficult to answer. Not so very long ago Sir William Crookes prepared a specimen of pure uranium, and he found it was almost inactive. And thus, for the moment, it seemed that uranium had no special powers of its own. Next the question was reopened by M. Becquerel, who discovered that specimens of uranium, deprived by him of their radio-active powers, were as active as ever eighteen months afterwards. To-day the question is — Is not uranium the parent of radium?

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[1] The question—Is radium an element? is discussed later.

This, then, is the story of the discovery of radium. We know where this substance occurs, and how to obtain its salts in an almost pure state. We know that it has a characteristic spectrum, and exhibits other properties similar to those of a well-known group of elements. We are aware of its astonishing radio-activity, and we know a good deal about its radiations, as will presently appear.

In the early days of radio-chemistry physicists and chemists were struck, as we have seen, by the resemblance of the Becquerel or uranium rays to the Röntgen or X-rays which all have heard of. The former seemed to have all the properties the X-rays possess, such as photographic activity, the power of making gases conduct electricity, the power of causing the formation of fog in moist air, and also, as it turned out, though this was not recognised at first, they can neither be reflected nor refracted like ordinary light. But as time went on it was discovered that the Becquerel rays possess yet other qualities which remind us of the "kathode rays" of a Crookes' vacuum tube. And thus the subject seemed likely to throw light not only on such important subjects as the Röntgen rays and kathode rays, but possibly even, as was soon pointed out, on the constitution of matter itself. The full consideration of this important aspect of our subject must, however, be reserved. At present we can only glance at it hastily.

Matter, according to chemists, is made up of minute indestructible particles called atoms—particles so small that millions of millions of millions of them can be introduced simultaneously into a vessel many times smaller than a lady's thimble; and the lightest of these atoms, and therefore the smallest particle of matter, according to the chemists, is the atom of hydrogen.

But physicists tell us that still smaller particles than atoms exist in the rays discovered many years ago by Sir William Crookes which stream off from the kathodes of very highly exhausted vacuum tubes. These rays carry negative charges of electricity and are deflected by magnets. Therefore, if electricity requires matter to carry it, it follows that the kathode rays contain minute particles. These particles of radiant matter are called "electrons." They have been the subject of much study at Cambridge and elsewhere, and Professor J. J. Thomson tells us that the mass of an electron is only a very small fraction of the mass of an atom of hydrogen.[1]

But what have electrons to do with radium? How do they come into this galley? We shall see in a moment.

We have already learnt that radium rays are not homogeneous. There are rays which will pass through cardboard, for example, and rays which cannot do this. And Professor E. Rutherford, of Montreal, a great authority on the subject of radium, tells us that rays of three distinct types may be distinguished in the radiations of radium. First the α-rays, as they are called, which are only deflected under the influence of a very powerful magnet, and then only to a small extent, and are easily absorbed by matter.

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[1] See Constitution of Matter, p. 26.

Secondly, the β-rays, which are readily deflected by magnets, and somewhat less readily absorbed by matter. And thirdly, some very penetrating rays which are not deflected by magnets, and which are called the γ-rays.

By far the greater part of the energy emitted by permanently radio-active matter is in the form of the α-rays—the β-rays only accounting, in fact, for a small percentage of the whole—and three things have been made out pretty clearly about them. First, that they carry positive charges of electricity, and therefore consist of particles; secondly, as Sir William Crookes has shown, that they are the cause of the beautiful scintillations of radium (described on a previous page), for the scintillating radiation, it will be remembered, will not pass through a card; and, thirdly, as we learn from Sir Wm. Ramsay and Mr. Soddy, that they are probably atoms of helium. On the other hand, the β-rays are found to consist of minute, negatively charged particles, which are similar in all respects to the kathode rays, and move, when not impeded, with velocities not very much less than that of light. In short, the β-rays are the "electrons" of the Crookes' tubes.

Now, at last, it will be seen, we begin to get some light on the "mystery of radium." Not very much, perhaps, but, at any rate, a scintillation. Radium is in a state of change, it is undergoing a process of spontaneous disintegration. If the astonishing qualities of radium are due to its throwing off swarms of particles, then, however small these particles may be, the supply of particles from any given specimen of radium cannot be inexhaustible, and there must be a limit to the period during which its radio-activity could be maintained. On the other hand, though these two points seem definitely settled, we have not yet traced to its source the vast output of energy which accompanies the disintegrations of radium, nor settled the great question raised by the occurrence of those disintegrations—Is the production of helium from radium due to the transmutation of an element, or can it be accounted for by some more commonplace explanation?

RADIUM AND THE INTERPRETATION OF RADIO-ACTIVE CHANGES

WE have seen in the previous essay how Madame Curie endowed chemistry with a new method of research and revealed to us a group of substances more wonderful and more interesting than any that have been discovered since the isolation of phosphorus by Brand in 1670. Many of my readers will remember, further, how the general interest in these new substances was intensified when Sir Wm. Ramsay and Mr. Soddy, by means of the spectroscope, recognised helium among the products of the disintegration of radium, and thus led many to declare that, after all, the transmutation of the elements might prove to be no mere dream of the alchemists, but a sober reality and one of the realised assets of the twentieth century? Finally, we all know that to-day the new Cambridge School of Physicists bids us grind some of our most cherished images into dust, and adopt in their place ideas about the nature of matter, ether, and electricity which seem well calculated to make the sober-minded philosophers of the last two centuries turn in their graves. Great discoveries and new ideas such as these are bound to make themselves felt over all the fields of science. I propose in this second essay on radium to give some account of the effects of these epoch-making researches and novel ideas on the particular department of science known as chemistry.

For nearly three generations the great conception of Dalton, which has been described as "one of the three keynotes of modern science," that "the ultimate particles of all simple bodies are atoms incapable of further division, and each of them possessed of particular weights which may be denoted by numbers," has served to interpret every great advance in chemical knowledge. Is this conception still acceptable to-day, in the sixth year of the twentieth century? And, if not, how does the fundamental theory of chemistry stand at this moment? These are some of the questions discussed in the following pages.

There are two ways of reading a novel. Every one knows that. You may begin upon the first page and go forwards, or you may begin upon the last page or the last chapter and work your way backwards. Those who adopt the latter mode sometimes make a new start after a while, cut the first pages of their book, and read it honestly through—a thing they might never have done had they begun at first at the beginning, and so I think perhaps the practice may be defended. But however that may be, I am going to ask my readers to excuse me if I begin this story at the wrong end, and plunge almost at once into what Professor Rutherford told the Royal Society about radio-activity and radio-active substances two years ago in his illuminating Bakerian lecture, which was delivered, I may remark, almost on the centenary of the birth of the atomic theory.[1] This plan will be the more convenient because the Bakerian lecture and its author's subsequent writings sum up what has been done on our subject, so far, at least, as the work concerns us at this moment.

It has frequently happened, as I have pointed out in another of these essays, that great advances in science have been intimately connected with the invention of a new instrument or with the improvement of an old one. Thus the spectroscope first made it possible for astronomers to study seriously the chemical composition of the sun. Liebig's invention of a trustworthy method of analysing compounds such as those which occur in the bodies of plants and animals laid the foundations of the greatest department of chemistry, the so-called "organic chemistry." And the weighing of molecules first really became practicable over a sufficiently wide field after Dumas had worked out his method for comparing the weights of equal volumes of the vapours of volatile liquids and solids. In the case before us we have an almost equally striking instance, for much of the work discussed in this essay, and much more of equal importance, could not have been carried out so readily, but for improvements which have made the gold-leaf electroscope an instrument of considerable precision, and a part of the necessary equipment of the chemical laboratory. It may be helpful, perhaps, if I briefly describe this instrument and the method of using it.

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[1] Dalton first made his ideas on this subject known in the course of a verbal communication to his friend Dr. Thomson, of Glasgow, on August 26, 1804.

The gold-leaf electroscope, in its simplest form, consists essentially of nothing more than two strips of gold leaf suspended inside an inverted glass shade by means of a metallic rod. The rod, well insulated by means of sulphur, is carried by the ebonite cover of the shade, through which it projects externally, and usually terminates in a knob, or a flat, circular plate of metal. Given this simple instrument and a large stick of sealing-wax, you may take your first steps as an investigator of radio-activity. The material to be experimented upon, unless you can borrow a little radium, will cost you more than the electroscope itself, but need not cost you much, as only a very minute quantity would be required for the simple experiments needed by many of us, as by Faraday himself, to fix our ideas and give reality to our notions concerning the use of a new or unfamiliar instrument.

If you charge your rod of sealing-wax with electricity by rubbing one end of it with a warm, dry flannel, and then bring the charged end near the knob of the electroscope the gold leaves will suddenly fly apart, but they will collapse again if you take away the sealing-wax. If, however, you touch the knob for an instant with a finger while keeping the electrified sealing-wax in position, and then remove the latter, the gold leaves will remain apart, standing out at a definite angle, which you can measure with the aid of a paper scale. The gold leaves stand out thus because they now carry similar charges of electricity, and these repelling one another force and keep the leaves asunder. If the leaves are well insulated they will remain apart for a considerable time, and, indeed, may collapse so slowly that you can detect no motion at all; but if you touch any part of the metallic rod when they are charged, the electricity will escape through your body to the earth, and they will collapse instantly. The same result will follow if you bring a fragment of a radium salt, or any other strongly radio-active substance, near the knob of the electroscope, for all such substances ionize the air—that is, make it a conductor of electricity—and thus enable the charge carried by the gold leaf system to escape at a more or less rapid rate, according to the ionising power of the material and its proximity to the metallic conductor. Radio-activity was originally discovered and for some time studied by means of the sensitised films used in photography, but these now are usually replaced by instruments such as the electroscope and the electrometer, partly because all types of radiations do not come into play when sensitised films are employed, and partly because the latter are not very suitable for quantitative experiments. It need hardly be added that various precautions must be taken and highly sensitive instruments employed in order to do work of the highest class.

I think I may venture to assume that my readers are more or less familiar with some of the leading facts about radium and its companions: that they know, for example, that radium occurs in minute quantities in pitchblende; that it is always found in company with the more plentiful element uranium; that, chemically speaking, it is allied to calcium, the metal present in limestone and quicklime; that it exhibits a definite spectrum, and, therefore, has been ranked as an element in the ordinary sense in which the chemist uses that term; that its atom is one of the heaviest that has been met with; that it gives off plentifully certain radiations which exhibit wonderful powers of generating light and heat, render various minerals phosphorescent and cause the air to conduct electricity; and that it emits an emanation, of which more anon. Finally, that it does these things for long periods without any perceptible diminution of its powers, and will, it is calculated, continue to do them for thousands of years before its powers will approach exhaustion. Therefore about many of them I need say no more. But the study of the radiations and emanations of radium and its fellows has led to results of deep interest to the chemist, and about these there is much to say.

As I have explained, it has been established for some time that the radiations of radium are not, as was at first supposed, merely Röntgen rays, but are composed of rays of at least three types. First, there are particles as heavy as, or heavier than, atoms of hydrogen, the lightest gas, which carry positive charges of electricity and probably are atoms of helium. Secondly, there are particles, or corpuscles, often called "electrons," a thousand times smaller than hydrogen atoms. These particles carry negative charges of electricity, move with velocities which vary considerably and in some cases are only one-tenth less than that of light itself, and they are so penetrating that they escape readily from vessels made of glass, can pass through thin sheets of aluminium or copper, and, in fact, can only be imprisoned securely in well-closed vessels of thick sheet lead. And, lastly, there are radiations which are not to be distinguished from Röntgen rays, and never appear alone, but always in company with electrons. These last rays cannot be imprisoned even in a moderately substantial leaden casket.

The three types of ray are known respectively as the α-rays, the β-rays, and the γ-rays. They all ionise air, and, therefore, can be detected by means of the electroscope or electrometer. Owing to their unequal powers of penetrating alien matter, such as sheets of paper and metal, it is possible to separate rays of one type from those of another type, to a more or less satisfactory extent, by using filters made of suitable materials.

At a comparatively early stage in the study of the radio-activity of uranium and thorium a most striking fact was established about them. It was this—that by appropriate treatment we can deprive each of them of its activity, or perhaps I should say, divide each of them into active and inactive fractions. Thus, if you bring a fixed amount of a salt of uranium into a suitable position in the neighbourhood of a sufficiently delicate and charged electroscope, and deduce the ionising power of the salt from the rate at which the gold leaves move as the instrument loses its charge: then dissolve the salt, add excess of solution of ammonium carbonate, filter off the small precipitate which falls, recover the salt left in the filtrate by boiling it down to dryness, and measure the activity of each of the fractions thus obtained as before, you will find that the main portion of the salt—viz. that recovered from the solution—has retained little ionising power, but that the small precipitated portion is, in proportion to its mass, many hundred times more active than the original salt. On the other hand, and this is most significant, if you put the two fractions aside and re-examine them after a few months, you will discover that the uranium salt has become, again, as active as ever, whilst the intensely active precipitate will have lost nearly every trace of activity.

The above remarkable discovery about uranium and uranium X, as the active precipitate is called, was made a few years ago by Sir Wm. Crookes and M. H. Becquerel. About the same time similar observations were made concerning thorium by Professor Rutherford and Mr. Soddy, who found, moreover, that thorium not only yields a highly active substance, called thorium X, analogous to uranium X, but gives off in addition an active, elusive emanation which can be passed through plugs of cotton-wool, bubbled through water, and collected, with accompanying air, in receivers, as if it were a gas, without destroying its activity. This emanation loses its activity like uranium X and thorium X, but more rapidly, so rapidly, in fact, that only about 2 per cent. of its power remains after the expiration of six minutes. This fact serves to distinguish the emanation from thorium X, for the latter still retains 50 per cent. of its activity at the end of four days, which assures us that the emanation is not, as otherwise we might have supposed, merely the vapour of thorium X.

What can be the meaning of these singular phenomena? Are we to suppose that uranium and thorium when roughly handled become, as it were, fatigued, and recover their radio-activity only after prolonged rest, or must we regard them as mixtures of inactive and active substances? Consider the facts. We can separate a sample of either of these metals into a large, nearly inactive fraction, which gradually again becomes as active as the original material, and a very small but highly active fraction, which in time loses all its activity; while, in the case of thorium, there is produced, also, a gas—the emanation—which is radio-active but loses its powers very quickly indeed. Does it not seem clear that uranium and thorium, as ordinarily met with, are mixtures? That they consist chiefly of inactive, or almost inactive, substances which undergo continuous change, producing in the one case the radio-active uranium X, and in the other case thorium X, and that these, in their turn, undergo decay, leaving perhaps inactive residues, and also, in the case of thorium X, emitting the active emanation mentioned above? In short, are we not face to face with chemical phenomena of a new order?

If this view of the matter is correct, then two new questions at once arise. Do the changes described above tell the whole story, or is there more to follow? And, again, how do the radiations, the α, β, and γ rays, come in, how are they related to these changes?

One of the most striking facts about radio-activity is this. It is, so to speak, "catching." If you expose a glass rod, a bottle, a basin, a block of wood, or any similar object to a few milligrams of radium, you will find soon that they acquire in some degree the power of discharging an electroscope, like radium itself. Indeed, after working with radium for some time an experimenter finds that everything in his laboratory, the walls of the room, and worst of all, he himself, has become radio-active. Now, this is very troublesome, and probably has led in the past to mistakes of various kinds. The effect is not permanent, it wears off and becomes, in time, more or less negligible, but recent investigations seem to indicate that the last traces of "excited activity," as this phenomenon is called, are not dispelled completely in the case of radium in less than forty years.

Most of us know that a magnet induces magnetism in any piece of iron brought near it, even though they do not touch ; and again, that bodies can be charged with electricity, as we charge the electroscope in the experiment described above, by bringing strongly electrified objects near them. And so we might very easily make the mistake of regarding "excited activity" as an effect similar to these. In reality it is nothing of the kind, but is closely connected with emanations like the emanation of thorium. Uranium, which emits no emanation, produces no excited activity on objects placed near it. No activity is excited by radium or thorium if they are placed in a box provided with a secure cover, even though that cover consists only of the thinnest sheet of mica, but, on the other hand, a paper screen through which the emanation can pass does not act like screens of glass, metal, or mica. The amount of excited activity produced in any given case is proportional to the amount of emanation present, and a sample, say, of thoria (oxide of thorium) has but little power of producing excited activity immediately after most of its emanation has been removed. Finally, excited activity seems to be something deposited by the emanation, something possessing definite physical and chemical properties, for, on the one hand, if you expose a platinum wire to thoria in such a way as to render it radioactive, and then heat it strongly, you find that the excited activity can be driven from the wire, and deposited on cool objects placed near it ; and, on the other hand, experiments show that the activity is basic, soluble in some acids as the bases are, but insoluble in the alkali ammonia. Thus it seems clear that excited activity is due to substances, solid substances, deposited by the emanations, and is not the result of an inductive action like that of a magnet on a piece of steel. One property of matter has not been identified in the case of these substances ; excited activity has not been shown to have mass, for the quantities to be dealt with are so minute that, at present, they are beyond the range of the most delicate balance.

Thus the plot thickens. Where a few years ago we had the single element "thorium," we find to-day, thorium, thorium X, thorium emanation, and excited activity, whilst the last substance, as we shall see, forms the source of still other members of this singular series.

I must now explain how it has been possible to recognise and sort out all these mysterious substances, in spite of the fact that most of them occur in quantities so minute that but for their radio-active powers they might never have become known to us. Let us suppose that we have samples of the emanations of thorium and radium—for radium produces an emanation—and that we measure their ionising powers at intervals of sixty seconds. If we do this,[1] it will not be long before we discover a marked difference between them. The activity of the thorium emanation will be found to diminish rapidly, so rapidly, in fact, that only the half of it will remain at the end of a single minute. In the case of the radium emanation the result will be very different. In this case the ionising power will remain so nearly steady that possibly no change may be detected at first. But if we lengthen the intervals between our observations sufficiently, we shall discover that the activity of this emanation falls to one-half its original amount in about four days. A repetition of the two experiments will give similar results, and if we extend our observations to other radio-active substances we shall find that the activity of each decays at a characteristic rate. In the case of radium, for example, the rate of decay is slow, so that only half of it would be transformed in about thirteen hundred years. In the case of uranium millions of years (6×10^8) would be required to produce the same result, whilst the emanation of actinium has so fleeting an existence that in less than four seconds the half of its power is already gone. Need anything more be said to show the value of this new, though difficult, method of research, this new gift from physics to chemistry? Is a fresh radio-active substance discovered? We have only to learn the law of the decay of its activity, and we shall know whether it is really new, or merely an old friend in a new guise. Do we wish to learn the effect of heating an excited activity from a given source to a high temperature? Then all we have to do is to render a platinum rod active by exposing it to the emanation which produces this excited activity, to measure the rate at which the activity decays, and then, after recharging the rod, to heat it to the desired temperature inside a cool cylinder, and afterwards measure the rate of decay of what remains on the rod, and also that of the portion which condenses on the cylinder, and we may hope to discover what we desire to know, although we can neither see, feel nor weigh the material we investigate.

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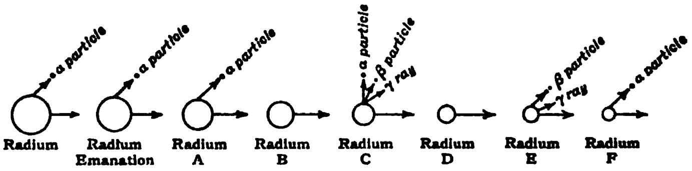
[1] It must not be supposed that the actual process is by any means so simple or free from complications as might appear from this brie reference to a really difficult and refined experiment.

When we carefully consider all the facts now before us—and we must remember that they apply more or less to all the substances mentioned here —the conclusion seems inevitable that these radio-active substances, uranium, thorium, radium, and actinium, are all of them, as already suggested, subject to continuous change. For some time after the thorium X has been removed from a sample of thorium the latter evolves but little emanation, whilst the thorium X, or rather the solution containing it, yields emanation plentifully. In the course of time the thorium recovers its power, and yields emanation as freely as at first, but we can again deprive it of this power by removing the thorium X, and we may repeat the process time after time. Is it not clear that the power of producing emanation belongs to thorium X, and that thorium X, in its turn, is constantly produced at the expense of the thorium? Again, excited activity is produced on bodies exposed to the emanation; it is not a gas like the emanation itself, but behaves, as we have seen, like a volatile solid. Can we doubt that the deposit which produces the excited activity is a product of the decay of the emanation? Is it not plain, on the facts before us, that in these products we have the outcome of a whole series of chemical changes—that thorium continuously produces thorium X, that thorium X emits the emanation, and that this in its turn leaves upon everything it touches a deposit which gives rise to the so-called excited activity? Remembering, again, that thorium can be separated from thorium X by the ordinary laboratory process of precipitation, that the emanation given off by thorium X behaves like a gas, and that the deposit which gives rise to excited activity is evidently a volatile solid, must we not conclude that the changes we investigate here, though they stand apart in many respects, have much in common with ordinary chemical changes, and consist, probably, in decompositions not altogether unlike those familiar to us in ordinary laboratory practice? Two characteristics, however, distinguish radio-active changes from ordinary chemical changes. First, the expulsion, during the progress of the changes, of particles carrying positive and negative charges of electricity (the α and β rays), and secondly, at any rate in the case of radium, as every one knows, the emission of a quantity of energy so enormous, considering the quantity of matter involved, that some eminent authorities have hesitated to admit that it can be derived from internal sources. It will give some idea of the importance of this last feature if I mention that it is calculated that a given volume of radium emanation gives out during its decay several million times as much heat as would be set free by exploding an equal volume of a mixture of oxygen and hydrogen to form water.

We must now turn our attention to the radiations, the α, β, and γ rays, thrown off during radio-active transformations. These are very important. First, on theoretical grounds, because, according to a modern view, we may suppose that the atoms of the chemist consist of more or less complex systems built up of positive and negative particles. Secondly, on practical grounds, since but for these rays and their power of ionising air, we might still be ignorant of the very existence of radium, and of its extraordinary properties. And, again, because by investigating separately the rates at which the activities corresponding to the various types of ray rise and fall in the case of the excited activity derived from each of the chief radio-active substances, Professor Rutherford has got together a body of evidence which has enabled him to make rapid progress with the analysis of these changes.

To give some idea of the method employed, I may remind my readers that, whilst all the radiations emitted by radio-active bodies can ionise air, and, therefore, can influence the rate of discharge of a gold-leaf electroscope or other similar instrument, they do not all penetrate obstacles such as sheets of cardboard, glass, or metal with equal facility, and that, consequently, rays of the three types can be separated from one another to a considerable extent, If, for example, at any stage in its decay radio-active matter causes the discharge of an electroscope when the matter is enclosed in a substantial leaden vessel, then we may be sure that it emits γ-rays, for only these can pass through lead ; whilst if a sheet of cardboard is substituted for the lead, it becomes possible for β-rays also to reach the electroscope. By taking advantage of such facts as these, it has been found possible to obtain curves indicating the decay of excited activity, as measured by given rays, in a number of cases, and from the study of these curves Professor Rutherford has arrived at highly interesting conclusions. He finds that the decay of an excited activity is by no means a simple phenomenon, but consists of a rapid succession of changes, each accompanied by the expulsion of particular rays, and each of which produces a characteristic product. He finds, for example, that by the decay of radium there is produced not only an emanation (there is no radium X), but also, by the decay of this, a whole series of distinct substances, which for the present are named radium A, radium B, radium C, radium D, radium E, and radium F ; radium D being a relatively stable substance which may be identified by the fact that no less than forty years would expire before the half of it would be transformed. As regards the accompanying radiations, he finds, first, that at the earlier stages α particles only are emitted. Secondly, that γ-rays seem to appear only in company with β particles, though β particles may occur alone. Thirdly, that all three seem to be emitted at one stage or another of the disintegration in most cases ; and, lastly, that rayless changes usually occur at some stage, though not always at corresponding stages. The following diagram, from Prof. Rutherford's book, shows the order in which the various changes occur, the nature of the product of each change, and also the rays which accompany each successive transformation. It will perhaps make the matter more intelligible than further words. It will at any rate indicate the complexity of the problem.

As will be seen, the smallest circles stand for the α-rays or particles, the small black dots for β particles, and the larger circles for the particles of radium, radium emanation, &c. The lives of these products vary exceedingly, the most stable being radium, which would still retain half its activity after thirteen hundred years; the least stable radium A, half of which would be transformed into radium B in three minutes.



On considering the statements made in the last paragraph, it will be seen that it becomes necessary to discover a parent for radium. A substance which disintegrates at so rapid a rate that almost the half of all that now exists will have disappeared in about a thousand years, evidently must have come into existence within a comparatively recent period. Not so much as one part in a million of the radium that existed in the world thirty thousand years ago can be supposed to remain to-day, and therefore the question arises, Where does the radium which now exists come from ?

For many reasons chemists are disposed to father radium upon uranium. Much that we know points to this. In the first place, the minerals rich in uranium are those which contain much radium. In the second place, uranium disintegrates far more slowly than radium, and thus the small proportion of radium in pitchblende is consistent with this idea. And, finally, there is reason to believe that the transformation of uranium into radium on a minute scale has actually been detected in the laboratory. If this conclusion be correct, it will be necessary, first, to extend the series illustrated by the above diagram by adding at least two more terms, viz. uranium and uranium X, and perhaps also several others. And then, since uranium itself is not eternal, but, on the contrary, disintegrates, though far more slowly than radium, to start on yet another quest, and attempt to discover the ancestors of uranium also.

If radium is an element, and consists of atoms, it must be admitted that these latest researches on the radio-active substances support the idea that the constitution of the chemical atoms must be very complex, and to that extent they are inconsistent with the atomic theory as formulated by Dalton. It will be noticed, however, that they do not weaken our belief that matter is discontinuous or atomic in structure, nor greatly affect the position of Dalton's theory in its application to chemistry. They do not suggest, for example, that atoms of the elements do not exist, nor do they forbid us to conclude that chemical combination and chemical decomposition respectively consist in the coming together and parting of atoms having fixed weights, as supposed by Dalton. On the other hand, they raise grave doubts in our minds whether the radio-active substances are really elementary in their nature. They offer, as Professor Rutherford has pointed out, no evidence to support the assumption which at first some were inclined to make, that helium is the final product of the transformation of radium; for many changes occur after the stage at which helium makes its appearance, whilst the expulsion of the *a* particles, which may not improbably be helium atoms, would leave a much heavier particle than an atom of helium behind them. If we suppose, with Professor Rutherford, that the *a* particles of radium consist of helium atoms, which agrees fairly, but not closely, with what we know about their masses, then it seems to follow that the α particles expelled from uranium and the other radio-active substances also must consist of helium. And if this be so, must we not conclude that these radio-active forms of matter are *compounds* of helium with some other known or unknown substances, and not *elements*, as has hitherto been supposed?

If we sum up what has been done, we must say, I think, that if the investigators of radio-activity have not yet definitely sapped the foundations of chemistry, nevertheless they have done truly great things. They have given us new and splendid methods of chemical research; they have introduced us to a class of reactions which are as novel as they are interesting and surprising, and they have enriched chemistry, it would seem, with a group of new and astonishing compounds, which, paradoxical as it may appear, are so stable that we cannot break them up by the chemical and physical forces at our command, and yet so unstable that they undergo spontaneously continuous disintegrations, accompanied, in one case at least, by the emission of energy on a scale which transcends all our previous experience. In a word, they have put into our possession a new domain, which is equally remarkable for its present yield and for its promise of future harvests.

CARBON AND THE SHAPES OF ATOMS

WHEN John Dalton visited London, Oxford, and Bristol a century ago to expound his new theory of atoms to a series of educated but non-chemical audiences, he took a step which his successors to-day usually endeavour to avoid under similar circumstances; that is, he carried with him, for the use of those who attended his lectures, a number of printed sheets, on which his ideas were expressed graphically by means of a system of chemical symbols. Dalton's idea, which proved sound, was that the symbols which had helped him to see his own notions clearly and definitely would help equally any other educated man or woman who might attempt to follow his arguments. With this fact to encourage me, I have ventured here and there in this essay to repeat the successful experiment of the founder of the atomic theory.

I do not think I need apologise for this, as I feel sure the few and comparatively simple formulæ in these pages will help every one who may read what I have written. But I have commenced my story with these few words of explanation, in the hope that they may prevent any one to whom the subject may seem attractive from putting the essay aside, after a superficial examination, with the idea that it is more difficult or more technical in its nature than the other members of the series of which it forms a part; for I feel sure that the exact contrary is the case.

In a single thimbleful of the "marsh gas" which occurs in every specimen of the "fire-damp" of coal-mines, there are about sixty million-million-million distinct particles or molecules. In each of these molecules of marsh gas there is one atom of carbon. What is the shape of this atom of carbon? It is my aim in this essay to show how far the chemists have got in the direction of winning an answer to this question, and, still more, to show how they have attacked the problem.

At first sight, perhaps, the question asked in the previous paragraph may not appear so very difficult, but when we remember the minute dimensions of the chemical atoms, and reflect that they are far beyond the reach of the most powerful microscopes, so that atoms are not to be isolated and looked at one by one, like crystals, it seems at once not simple but almost fantastic and impossible. Nevertheless, in a limited sense the problem has been solved ; and the key which has unlocked, or partly unlocked, the chamber in which the secret has long been hidden is called "isomerism." This key has been forged by many hands. Its construction may be said to have been begun by Liebig in the laboratory of Gay-Lussac, in a cellar, I believe, in the city of Paris, in the year 1823. And for the better information of those who think the members of the younger generation are always idle when they seem to do nothing, I may mention that Liebig appears to have started on the quest which resulted in the discovery of isomerism when dawdling as a boy in the market-place at Darmstadt watching a cheap-jack make the explosive constituent of fire-crackers by adding brandy to a solution of quicksilver in strong nitric acid.

The element carbon, so familiar to us in the form of coal, soot, charcoal, and diamond, which predominates in almost every part of the tissues of all plants and animals, stands out among the eighty elements on account of the astonishing number and variety of its compounds. No less than sixty thousand compounds of this single element have been isolated and studied by the chemists ; and although it is probable that our knowledge of carbon is still in its infancy, the compounds of this element already bulk so large in the book of chemistry that this volume would shrink, perhaps, to less than half its present dimensions if the record of carbon and its derivatives were removed from its pages. It will help, I think, to give an idea of the fecundity of this unique element if I mention, before we proceed, that it has been calculated that no less than eight hundred and two substances (called "tridecanes") may possibly exist, each of which would have distinct properties, and yet would be indistinguishable from the rest by chemical analysis, since every one of them contains the same proportions of carbon and hydrogen, and corresponds to the same chemical formula. When I add that very few of these eight hundred substances find a place among the sixty thousand carbon compounds known to chemists ; that nearly everything necessary for maintaining life, and many luxuries ; nearly all we eat, and, with the exception of water, nearly all we drink ; everything we wear ; every flower of the field ; every perfume, nearly every dye, and a thousand things besides largely consist of carbon compounds, all will agree that even the wonders of radium pale before the possibilities that lie hidden in a handful of soot or in a lump of common charcoal, and will understand why it is that during the last eighty or ninety years scores and hundreds of men have devoted their lives to the study of this single element and its transformations.

The idea conveyed to the chemist by the word "isomerism"—the idea, that is, that two or more distinct compounds may be built up by uniting the same elements in exactly the same proportions — came into chemistry somewhat haltingly.

It has, of course, been known from the days of Lavoisier and Smithson-Tennant that diamond, blacklead, and charcoal, if pure, though so different from one another in appearance and in many of their properties, all are elementary in their nature; and, what is more, all consist of the same element, viz. carbon. But the notion that similar phenomena might occur among compounds was not grasped at once even after an example of isomerism had been discovered among the compounds of carbon and hydrogen by Dalton and his friend Dr. Henry. This curious fact may be ascribed to the circumstance that those who accepted the atomic theory in its early days started with the idea that a given set of atoms, when combined, would be sure to produce one compound and no more, or, to put the same idea in another form, that the properties of a compound must depend solely on the nature and number of the atoms contained in its molecules.

It was not until after the year 1825 that wider and truer views began to assert themselves.

If you dissolve a bit of silver—say, a sixpenny piece—in a dessert-spoonful of strong nitric acid, apply a gentle heat, add to the liquid while hot rather more than twice its weight of spirit of wine, continue to heat the mixture for a short while, and then allow it to cool, small brilliant, colourless crystals in thin plates will presently separate from the solution. These crystals, which can only be preserved safely in extremely small quantities or under water, consist of the silver salt of fulminic acid,[1] and are so unstable that when heated, rubbed in a mortar, or touched with oil of vitriol they explode with the utmost violence, giving off great volumes of gas and leaving a residue of silver.

About the year 1824, when working with Gay-Lussac, Liebig succeeded in analysing this dangerous salt, and, owing to the ideas which then prevailed among chemists, he was not a little surprised to find that it consisted of the same elements, united in the same proportions, as the silver salt of another acid, then known as cyanic acid, which was analysed, about the same time, by his fellow-countryman Wöhler. Now the substance analysed by Wöhler possessed no explosive qualities whatever. It was as unlike a fulminate as anything well could be. Hence Liebig's analysis created not a little excitement. How could two substances so different from each other consist of the very same elements united in the same propotions? The idea seemed absurd. It almost savoured of alchemy, which was much more out of fashion in those days than it is just now. But repeated analyses only confirmed Liebig's discovery; presently his greatest opponent, Berzelius, himself discovered another case of a similar kind, and by the year 1830 it was agreed, at the suggestion of Gay-Lussac, who from the first had supported his younger colleague, that the properties of chemical compounds do not depend solely on the nature and number of the atoms which build up their molecules, but also on the manner in which the atoms are arranged within the molecules. About a year later Liebig completed the working out of his beautiful method of organic analysis (in the course of which process he discovered how to make bicycle tyres—that is, tubes of india-rubber), and thus the new organic chemistry was started on its triumphant career. I have sometimes wondered if, by any flash of inspiration, Liebig ever in any degree foresaw the results that were to flow from these two researches—the new ideas; the vast additions to knowledge, amounting, in effect, to the creation of a new branch of science; the historic industries about to be destroyed; and the new industries about to be created, to the great and special gain of his own fatherland?

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[1] The mercury salt of this acid is the explosive material used for charging percussion caps.

When Berzelius introduced the term "isomerism" into chemistry the known cases of this phenomenon were still but few. Dalton and Henry had discoverd two isomeric substances among the compounds of carbon and hydrogen. Liebig had demonstrated the identical composition of silver fulminate and silver cyanate. Wöhler had effected a revolution by changing ammonium cyanate into urea by the action of heat.[1]

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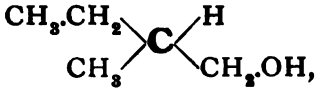
[1] Before this achievement it was commonly supposed that compounds like urea were produced under the influence of a special "vital force," and that no such compound could be prepared from its elements in the laboratory.

Berzelius had established the existence in grape juice of racemic acid, an isomeride of tartaric acid, and that was all. To-day cases of isomerism have been discovered by hundreds, and the whole subject is reduced to such order that the skilled organic chemist can predict the existence of undiscovered compounds, foretell their properties, and suggest their probable modes of formation with a degree of accuracy almost equal to that of Mendeléeff when he predicted the existence and foretold the properties of gallium and germanium by means of the famous "periodic law."

From the earliest days since the introduction of the atomic theory into their science chemists have used a conventional system of symbols to assist them to picture chemical changes and the constitutions of chemical compounds; and, as I have already said, there is no doubt that this "chemical algebra" did much to bring Dalton's ideas into favour by making them more easy to understand than they would have been without this assistance. Dalton's symbols, which represented the atoms by circles bearing various devices to distinguish them one from another, have been altered a good deal since their inventor passed from the stage, and they now take the form of letters of the alphabet arranged according to a few simple conventions. No doubt they seem a little alarming to a beginner at first sight, but they are not really difficult to follow, and such a formula as

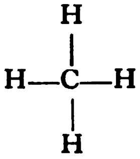


which represents one of the constituents of the fusel oil of pot-still whisky, and rejoices in the not very attractive name of methyl-ethyl-carbin-carbinol, soon becomes quite a lively and suggestive affair to the student of chemistry. In this and all similar formulæ each letter represents an atom, of fixed weight, of an element ; C standing for an atom of carbon, H for an atom of hydrogen, and O for an atom of oxygen. Numerals, like the figure 3 in the group CH_3, indicate the number of atoms of the given element in that group. Thus CH_3 means that here we have one atom of carbon united with three atoms of hydrogen.

I should add that it is found desirable to treat certain groups, for example, the group CH_3, the group CH_2HO, and, again, the group OH, as if they were indivisible, like atoms, since these groups can, in fact, be transferred unbroken from one molecule to another in chemical changes almost as readily as atoms themselves. These groups are called "compound radicles," to distinguish them from atoms or simple radicles. The reader will now see that the above formula tells the chemist at a glance that the substance it represents may be considered to consist of four separate groups of atoms—namely, methyl (CH_3), ethyl (CH_3CH_2), hydrogen (H), and the group CH_2OH—all gathered about a single carbon atom, which, to distinguish it from the rest, is printed in heavy type. The chemist knows how various radicles can be got into the molecules which contain them, and also how they may be removed again from these molecules, and a formula like the one we are considering may convey to him a great deal of other information, which does not concern us now.

One of the most striking qualities of the carbon atom is this, that whilst other atoms show, as a rule, relatively little inclination to combine with atoms of their own kind, carbon atoms, on the contrary, will unite with one another literally by the dozen to form stable compounds. Indeed, one compound, dimyricyl, $C_{60}H_{122}$, may be quoted which contains no less than sixty atoms of carbon joined together, as we believe, by chemical attraction, whatever that may be, more or less like the links of a chain.

There is one other point I must mention at this stage. No single carbon atom has ever been found to be combined with more than four atoms of hydrogen, and even this only occurs in the case of marsh gas, which accordingly may be represented, graphically, as follows :—



$$\begin{array}{c} \mathrm{H} \\ | \\ \mathrm{H}-\mathrm{C}-\mathrm{H} \\ | \\ \mathrm{H} \end{array}$$

The lines joining the symbols of the atoms of carbon and hydrogen in this formula represent the units of the attraction which holds the atoms together, a single carbon atom, as you will see, being supposed to have as many of these bonds as all four hydrogen atoms taken together. Of course no such bonds have really been proved to exist. Like the letters C and H they are mere symbols, used to fix our ideas.

Marsh gas, as we have seen, is by no means the only compound formed by the union of carbon and hydrogen. On the contrary, this substance forms the starting-point of a large and important series of compounds, known as the "normal paraffins," whose members form the chief components of the thousand million gallons of petroleum, or rock oil, obtained every year from the oil springs of North America. Let us see how this can be.

If we think of carbon and hydrogen atoms respectively as solid objects, like, let us say, minute billiard balls, which attract one another, but differ in their other properties, having, for example, different weights, and unequal powers of combining, so that a single carbon atom can unite, as indicated in the diagram on the previous page, with no less than four hydrogen atoms, we shall see that if two carbon atoms were joined together each would use up at least one of its attractions, or, as chemists say, one bond,[1] or one valency, and, consequently, that the two atoms of carbon, when so combined, would still be able to attract six atoms of hydrogen, but not eight, as two separate atoms of carbon can do? If this argument be sound, it follows first that, in addition to marsh gas, there might exist a second compound of carbon and hydrogen, containing in each molecule two atoms of carbon united together and six atoms of hydrogen, as indicated by the figure given below:—

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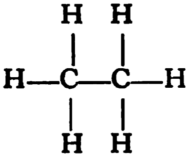
[1] The term "bond" applies to the lines joining the atoms in the above formula, and is supposed to symbolise a unit of attraction.



$$\begin{array}{ccccc} & H & & H & \\ & | & & | & \\ H- & C & - & C & -H \\ & | & & | & \\ & H & & H & \end{array}$$

Secondly, as you will discover, if you draw the necessary diagram on paper, that there might be a third compound consisting of a sort of chain made of three atoms of carbon together with eight atoms of hydrogen, a fourth based on a chain made of four atoms of carbon, a fifth with five, and so on, theoretically, *ad infinitum*. In reality, however, only a score or so of the compounds that we could discover in this way with the aid of a pencil and a bit of paper have been prepared and studied, but many of these are very familiar; for cleaning oil or benzoline, the paraffin oil used for lamps, the oil used for lubricating bicycles and other machinery, vaseline, and the paraffin wax of which candles are made, all are mixtures of hydro-carbons belonging to this interesting series.

Now this method of inventing new compounds may seem very simple and straightforward. But if you take pencil and paper and write down the picture formula of the third member of the series we are investigating, and then that of its successor, you will find that a new and puzzling complication arises at this point. It is this: You can arrange four atoms of carbon in two different ways. You can set them side by side, like four billiard balls placed in a row, or like the links of a chain, so that no single one of them is connected to more than two others, viz. to one on its right hand and to another on its left. Also you can arrange three of the atoms of carbon (and an atom of hydrogen) round about the fourth atom of carbon, like fixed stars, or like the four hydrogen atoms in the figure on p. 271, so that one of the four carbon atoms is connected to each of the other three and serves as a sort of nucleus to hold them together. Thus we can invent two butanes, as these compounds would be named, in which the atoms are arranged dissimilarly within their molecules—that is to say, *two isomerides*. If we pursued this idea further we should find that with five atoms of carbon at our command we could invent three distinct substances, with six carbon atoms, five, and so on, the number of theoretically possible isomerides increasing so rapidly as we proceed that with thirteen carbon atoms no less than eight hundred and two compounds can be invented on paper.

Now, curiously enough, only one marsh gas, only one paraffin containing two atoms of carbon in its molecule, and only one containing three atoms of carbon have ever been prepared. But, on the other hand, two butanes (C_4H_{10}), three pentanes (C_5H_{12}), and no less than five hexanes (C_6H_{14}), that is to say, just the number suggested by theory, have been prepared and studied in the laboratory. This, to say the least of it, surely implies that we stand on fairly solid ground when we invent new compounds in the manner suggested, even though as we go higher up the series we find that comparatively few of the swarms of theoretically possible compounds have been prepared and identified up to the present time.

It would be interesting, if it were possible, to pursue this part of our subject somewhat further, and to study some illustrations drawn from less simple groups of compounds, such as the alcohols, the class to which common spirit of wine belongs, or the ethers. But excursions into these wider fields are forbidden to us, partly from consideration of space, and partly because a greater variety of illustrations might produce confusion, by adding to the complexity of the subject, rather than give clearness to our ideas, and so tend to defeat the object we have in view.

If any of those who read this article should chance to be of a mathematical turn of mind, it may have occurred to them as they read the last two or three pages that, in spite of the support brought to the above speculations by the known facts of the case, the theory of isomerism, as stated here, exhibits a very serious defect. Is it possible, they will ask, that an hypothesis which arranges atoms in molecules as if they existed on plane surfaces—that is, in space having length and breadth but no thickness—can give an adequate picture of the real atomic construction of a molecule? Now this criticism is perfectly sound. These models or diagrams which we have constructed are all very well in their way, but there are facts they do not account for, as well as the facts they do account for. It happens, however, that our road leads through these models and not to these models. They mark a stage in our journey but they do not bring us to our destination, nor even within sight of our destination, as we soon shall see.

If you draw a circle or some other simple figure on a sheet of white paper and look at it through a crystal of Iceland spar, you will see not one circle, but two, because each ray of light that enters the crystal is split into two rays, which emerge from the crystal separately and thus give two images. It is possible by using suitable apparatus to get rid of one of these two rays, and when this is done we have what is usually spoken of as polarised light—that is, light consisting of vibrations which are all in a single plane transversely related to the line of propagation of the beam of light. These vibrations all might be vertical vibrations, or horizontal vibrations, but they would not consist of a mixture of the two. Now certain substances—for example, sugar—act in a peculiar manner on polarised light. If, for example, you arrange an apparatus so as to obtain a beam of polarised light, place in its course a specimen of one of these substances, and study the light as it emerges from the substance, you will find its plane of polarisation has been twisted or rotated a little to the right or a little to the left, the effect produced being proportional to the thickness of the layer of substance, or, in the case of a solution, proportional to the length of the column of solution and to its concentration so accurately as to afford us a very useful method of measurement.

The substances which, like sugar, behave as described in the previous paragraph are frequently spoken of as "optically active," those which twist the plane of polarisation to the right being said to be right-handed or "dextro-rotatory" and those which have the opposite effect left-handed or "lævo-rotatory," and they fall into two distinct classes—first, solids, like rock crystal, which lose their optical activity when they are melted or dissolved, and, secondly, substances which are active both when melted and when dissolved in water or some other solvent. The former class we may neglect, since their activity would seem to depend on the way in which the molecules are arranged to build up their crystals, but the activity of the latter class is most important to us. It brings us, in fact, to our most interesting case of isomerism.

About sixty years ago Mitscherlich, the chemist, communicated a memoir to the Academy of Sciences in which the following words are to be found: "The racemate[1] and tartrate of soda and ammonia have the same chemical composition, the same crystalline form, the same angles, the same specific weight, the same double refraction, and consequently the same inclination of the optic axes. Dissolved in water their refraction is the same. But while the dissolved tartrate causes the plane of polarisation to rotate the racemate exerts no such action. . . . Here the nature and the number of the atoms, their arrangement, and their distances apart are the same in the two bodies." This note put the chemists of those days in a difficulty. "How could it be admitted that the nature and number of the atoms, their arrangement and distances apart in two chemical substances are the same, without concluding that the two substances are absolutely identical?" The identity of the two salts, asserted by Mitscherlich, and the differences in their optical characters were felt to be incompatible. One or the other must be wrong. We might, it is true, get round the difficulty by supposing that there are in solutions of these salts not single isolated molecules but groups of molecules, and that the optical properties of the solutions depend on the arrangement of the molecules which make up these groups. But we have no right to accept this royal road out of the dilemma until every other possible solution of the problem has been tested, or before we have attempted to carry the theory of isomerism at least one step further, and endeavoured to replace the imperfect conceptions which deal with molecules as if they consisted of atoms arranged in space of two dimensions by a theory of atomic arrangement in three-dimensional space.

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[1] See cases of isomerism, p. 267.

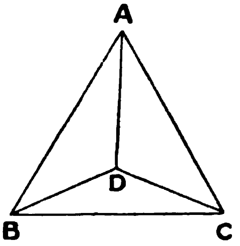
These considerations, or considerations very like them, impelled Louis Pasteur, who was a student in the Ecole Normale when Mitscherlich published his note, to refuse a professorship at Tournon in order to study the crystals of the racemate and tartrate of soda and ammonia. The result was that before long he noticed a fact which all his predecessors had failed to detect—namely, that the salt of racemic acid when crystallised from an aqueous solution deposited two distinct sets of crystals, further, that these were so related that when specimens of one variety were placed before a mirror they produced images exactly like the crystals of the second variety, and, most wonderful of all, that the members of one set of these crystals were quite indistinguishable from those of the salt prepared from ordinary tartaric acid; whereupon, to cut a long story short, by separating the two kinds of crystal, by hand-picking, into two heaps and regenerating their acids, Pasteur was able to demonstrate that a sample of racemic acid can be split up into two acids—first, ordinary tartaric acid, and, secondly, a new acid optically unlike tartaric acid and unlike racemic acid also. This new acid was not only optically active, like tartaric acid, but optically active in the opposite sense, for whilst tartaric acid rotated polarised light strongly to the right the new acid rotated it equally strongly in the opposite direction. Racemic acid, in short, was not identical with tartaric acid in the structure of its molecules, as Mitscherlich had supposed, but quite unlike it. It consisted, in fact, of a combination of tartaric acid with another acid which differed from it most notably in its optical properties.

These remarkable phenomena so difficult, indeed, so impossible to account for by formulæ constructed in two dimensions—which have been fully confirmed and supplemented by many other facts of a similar order—suggested, from the first, to Pasteur the idea that they must depend in some way on a want of symmetry, a want of balance in the arrangements of the atoms within the molecules of active compounds, and convinced him that in these two acids "we have to do with an asymmetric arrangement the images of which cannot mutually cover one another." And he endeavoured to conceive arrangements of atoms in gyratory spirals, and at the angles of irregular tetrahedra, that might be consistent with this idea. But partly because the necessary data were not available in those days, and partly, perhaps, because, Pasteur's attention was diverted to other fields, it was not until a good many years later (1874) that van't Hoff and Le Bel by their splendid conception of the *asymmetric carbon atom* carried chemistry at a bound into the regions which Pasteur and others had endeavoured in vain to penetrate.

Pasteur's famous research greatly stimulated the investigation of the optically active compounds, and so led in the course of years to the gathering together of a body of information which was not at the disposal of the earlier workers. It was found, for example, that the case of tartaric and racemic acid did not stand alone, that optical asymmetry usually manifests itself in the shapes of the crystals of active substances like the two tartaric acids, the isomers showing differences in crystalline form by which they may be distinguished almost as readily as by their optical qualities. Substances of this class were found, moreover, to be less closely alike, even in their chemical qualities, than Mitscherlich had supposed, so that a pair of optical isomers may differ so completely in their physiological relations that one of them may be selected and the other rejected as food, if both are placed in the feeding grounds of some of the lower organisms. But the most important discovery of all was this:

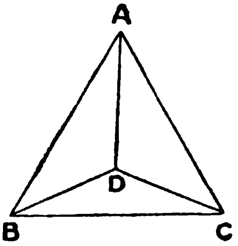
In every optically active carbon compound we find at least one atom of carbon joined, like the carbon atom whose symbol is leaded in the formula written on p. 269, to four quite distinct groups or radicles.

Now an atom of carbon thus laden with four diverse groups of atoms is termed an *asymmetric carbon atom.* And van't Hoff and Le Bel have shown that an atom of carbon so loaded may be represented as a solid in two different ways, which have similar relations to one another as those of an object and its image seen in a mirror.

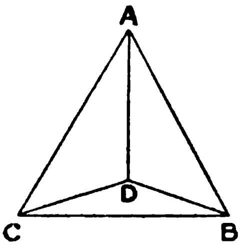


To realise this we may suppose that a carbon atom is a tetrahedron and the four diverse groups of atoms are held at its four corners, or, as is more usual, assume that in combination the four groups of atoms are placed at the four angles of an imaginary tetrahedron, whilst the carbon atom is placed at its centre, as in the above diagram, where for the sake of simplicity the letters A B C D are intended to represent the four diverse groups which exist in an active molecule.

If you were to place a solid model, I., corresponding to the above figure, opposite a mirror and look at its image II., you would see the latter as drawn below.



I.



II.

Now here we have representations of two molecules containing the same radicles, A, B, C, D, similarly arranged about an atom of carbon. And yet there is a difference between these molecules. They are not identical, and it would be impossible, as you will find on a trial, to make either of them, as a whole, coincide exactly with the other in any position whatever. You cannot make A in figure I coincide with A in figure II, and at the same time make B, figure I, coincide with B, figure II, and also C with C and D with D. The two molecules are not superimposable. That is to say, they do not represent identical substances, but correspond to the "asymmetric arrangement the images of which cannot mutually cover each other" which Pasteur sought in vain half a century ago. They seem to give us, moreover, a hint about the shape of the atom of carbon. Not a clear indication certainly, but a hint such as one might get about the shape of a distant rock from one peep at it from one single point of view during its momentary illumination by a flash of lightning. And that, I fear, is all we can hope to attain at present.

It must not be supposed that stereochemistry rests where this essay leaves it. During recent years a real beginning has been made in laying the foundations on which, in time, there may be built up a stereochemical scheme wide enough to embrace all the elements known to chemists. Nor must it be supposed that the completion of such a "newer chemistry" as that which is foreshadowed by these latest speculations and researches on the constitutions of molecules will ring down the curtain on chemical research in this direction ; for nothing seems less likely than that. Is it possible to believe that the atoms in any single one of the molecules of which all matter is built up are held rigidly together in assigned positions in the molecule in the manner suggested even by the most advanced of the "mind pictures" set out in this article? Can we believe for a moment that the atoms which compose a molecule stand in place, fixed, immovable, like the pieces on a chess board, till a chemist or some other agent intervenes to set them in motion and produce a new arrangement —that is, a new chemical compound? Does it not seem far more likely that, like the pieces in that unique game of chess in which Alice and the Red Queen took part through the looking-glass, the atoms move within the molecules, perhaps like the members of the heavenly systems—or as suggested years ago by the late Professor Williamson, that they may even move from molecule to molecule in an unending course of migrations? If so—and facts are not altogether wanting to support ideas of this kind—then stereochemistry will not say the last word on this interesting subject, and stereochemical formulæ will have to be replaced sooner or later by living pictures, for which models may perhaps be found in the constellations which glorify the heavens.

FERMENTS AND FERMENTATIONS

NOTHING, perhaps, is quite so interesting to the chemist as that borderland which separates his science from the domains of the biologist, where he approaches, from afar, one of the biggest questions of all, and asks himself! "What is the connection between Life"—"the organism in action," as Helmholtz defined it—"and the physical and chemical manifestations which are invariably found in its train?" How are the vital processes related to those physical and chemical phenomena which are their inseparable companions, which seem to be of their very essence, and yet are found so distinct, so indescribably different, the moment we bring them into comparison?

In the Middle Ages Paracelsus and the medical chemists sought to reach the answer to this question by various short cuts: by making hypotheses about the powers of the Archæus, the ruler of the stomach, who changes bread into blood, and separates the poisonous from the nutritive part of food. Later, their successors tried to win the secret by inventing "vital forces," which were supposed to exert power over matter by means of the chemical and physical forces, and yet to be able to control these forces, suspending and permitting their action at will. But now these royal roads have been abandoned as completely as are the teachings of the Alchemists about the virtues of the philosopher's stone, and to-day we attack the problem by the slower but surer methods of the experimental sciences.

Perhaps no part of the borderland alluded to above has yielded richer harvests than those we have reaped from the study of the ferments and fermentations. One has only to recall what Louis Pasteur and his colleagues have taught us about the nature of disease, what Lord Lister has done for surgery, or what is being done to-day in Africa and elsewhere to extirpate malaria to realise how splendid these harvests have been. It is not, however, on account of its practical importance that I have selected the subject of ferments for this essay; but rather because its history indicates so well how we may approach, even if only from a great distance and in a tentative way, the important question which stands at the head of these pages.

But do not expect to find a definite answer to this question here. Between that question and the answer there is a great gulf fixed, and, as we shall see, we are, as yet, only gathering the materials for building a bridge. After all, however, is it not as interesting and instructive to watch the making of great girders in a factory as to look at the bridge builders putting them in their places afterwards ? I think it is.

Although the surface phenomena of fermentation have been familiar for centuries upon centuries in the leavening of dough, the fermenting of the juice of the grape, the souring of wine when it is exposed to the air ; and, again, in those processes of putrefaction and decay which remove from the surface of the earth the remains of all that has lived and is dead, preparing them to serve once more as the food of plants, and thus fitting them again to play their part in making the glory of the earth, yet the science of the ferments is of such recent growth that we may be said to owe it to the latter half of the last century. This, perhaps, was inevitable, for though the outside phenomena of fermentative change were so much in evidence, there were at least two great obstacles to prevent our ancestors from studying the ferments with such success as that which they attained, for example, in mathematics and astronomy.

Ferments and fermentations are, as we know now, intimately connected with the activities of the minute living cells which form the most important part of every organism. Hence, in the first place, it was scarcely possible to make a real start before the microscope had been brought to a considerable degree of perfection, or whilst the value of that instrument was still very imperfectly appreciated.[1] Secondly, until comparatively modern times our predecessors were handicapped by their ideas about the origin of life; as will be recognised when I recall the fact that Aristotle taught "that all dry bodies which become damp and all damp bodies which are dried engender animal life;" that Virgil believed that swarms of bees are generated in the corrupting entrails of bulls, and that, even as late as the seventeenth century, Van Helmont, one of the greatest of the medical chemists, was convinced that scorpions can be produced in closed vessels from decaying vegetable matter.

In spite of these and other obstacles, however, the early workers did something to prepare the way. The writings of Robert Boyle, who declared that he who should fathom the nature of fermentation would understand many things; those of his friend Dr. Willis, the eminent Oxford physician, who laid the foundations of the first physico-chemical theory of fermentation, by suggesting that ferments were bodies possessing internal motions which they were able to transmit to fermentable matter, together with what we read in the books of other writers, make it plain that for many years the importance of the subject had been well understood. One peculiarity of the ferments especially had been recognised in the case of leaven, viz. that it is possible to ferment an almost unlimited quantity of dough by means of an extremely minute quantity of leaven. It had been noticed also that, just as the rising of dough depends on the presence of the leaven, so vinous fermentation is always associated with a deposit, yeast, which sometimes floats as a scum on the surface of the fermenting liquid, and sometimes is deposited beneath it. Further, by the end of the seventeenth century, and perhaps earlier, the analogy which exists between putrefaction and the fermentation of sugar was pretty generally admitted. Thus by that time the term fermentation, which comes from *fervere* to boil, was no longer restricted, as it had been at first, to changes which are accompanied by marked effervescence.

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[1] The achromatic microscope of Tully was only invented about 1824, and probably members of the medical profession still live who can remember the time when it was difficult to find a good microscope in a London hospital.

To-day the list of known fermentations is vastly longer than it was a century ago. It includes, for example, the production of vinegar, or acetic acid, from wine; the changes by which cheese is prepared from milk; the action of the ptyalin of the saliva when it converts the starch of our food into sugar, and that of the diastase of barley, which we utilise in making malt, by which sugar is produced from starch during the germination of the grain. Other examples are to be found among the changes which occur when we prepare many vegetable dyes, such as indigo; in the tanning of leather; in many of the operations of digestion; and, to turn from useful fermentations to those which are malign, in the processes by which the deadly cadaveric alkaloids, the ptomaines, are generated in decaying animal matter, and in those which occur in the living tissues in many of the terrible diseases which afflict us. Though perhaps we do an injustice to the latter fermentations in terming them malign, since the primary function of some of these is the useful one of preventing dead matter from cumbering the earth's surface by promoting its decay, and they become malignant only when by misfortune or by mismanagement we fail to restrict them to their proper sphere of action.

Ferments are bodies, like yeast cells, which are capable of producing a variety of chemical changes in numerous substances, and which present the peculiarity that a very little of a ferment will go a very long way. A speck of yeast, for example, so small that it cannot be seen without the aid of a microscope, may be so used that tons upon tons of dough may be leavened, or hogsheads upon hogsheads of brewers' wort converted into ale or beer, by its aid. Since fermentations sometimes, and indeed often, start spontaneously in organic matter when it is exposed to the atmosphere, we must believe that some ferments exist among the particles of dust that we see floating in the air when a beam of light is projected into a dark room.

Science, as most of us know, is agitated every now and then by great and animated controversies—by discussions which sometimes even become bitter, through excess of zeal. Such a discussion on the subject of fermentation took place in the second half of the last century, between Liebig, the great German chemist, and

Louis Pasteur ; the former supporting a chemical, or physico-chemical, theory, the latter doing battle for the vitalistic hypothesis which ascribes fermentations to the actions of certain minute living organisms. Liebig's hypothesis was founded on an earlier one derived from Berzelius and Mitscherlich, which, in its turn, owed something to Dr. Willis of Oxford.

Long before Liebig's time it had been shown that the alcohol of beer and wine does not, as was supposed in the fifteenth century, exist ready made, though hidden, in brewer's wort or in the juice of the grape, but is formed during the fermentation. And by reasoning upon the results of analysing sugar and alcohol chemists had come to the conclusion that sugar, when fermented, is split up into two new substances, alcohol, and carbonic acid gas, the gas that is produced in combustion and by the respiration of animals. The question, therefore, was : How does the yeast produce this effect ? How does it bring about the decomposition of the sugar ? Does it act by contact as Dr. Willis and others had suspected, in virtue, perhaps, of the motions of its particles, or is its power due, as Cagniard de Latour taught in 1837, to the vegetative processes of certain little globules, detected in yeast as long ago as

1680 but since forgotten, which De Latour and others had shown to be living organisms probably belonging to the vegetable kingdom?

Liebig supported the first of these contentions, though he greatly modified the original theory. He did not deny that yeast was necessary for alcoholic fermentation, or assert that fermentation could take place in its absence. But he considered it to be merely the source of a supply of putrefying nitrogenous matter. Where there is life there is death also. Hence the presence of living yeast implied the presence of dead yeast, and it was in this dead yeast that Liebig found the source of its fermentative power. It is the universal experience that all organised bodies vanish after death. The remains of animals disappear quickly, those of vegetables less quickly, provided in both cases that air and moisture are supplied to them at the earlier stages. Liebig's theory of fermentation was founded on this. "It is obvious," said he, "that by the contact of these organic compounds with the oxygen of the air, a process begins, in the course of which their constituents suffer a total change in their properties. This change is the result of a change in their composition. Before contact with oxygen, their constituents are arranged together without action on each other. By the oxygen the state of rest or equilibrium of the attractions which keeps the elements together has been disturbed in a particle of the substance, and, as a consequence of this disturbance, a separation or new arrangement of the elements has been brought about." "The continuance of these processes even when the oxygen, the original exciting cause of them, no longer acts shows most clearly that the state of decomposition which has been produced among the elements of a particle of the mass exerted an influence on the other particles which have not been in contact with the oxygen of the air; for not only the first particles, but by degrees all the rest undergo the same change." According to Liebig, not only the yeast of the brewer's vat, but all putrescible nitrogenous matters when putrefying can transmit to fermentable substances the state of decomposition in which they themselves are. The vibration which has been communicated to their constituent particles by the disturbance of their equilibrium can be imparted afterwards to the particles of other substances which may come into contact with them. Thus putrescible matter alone, according to Liebig, could act as a ferment. And he considered the yeast was useful because it afforded the necessary supply of putrescible matter.

For a time Liebig carried opinion with him, and it was held widely that the activity of yeast was due not to its life processes, but to its tendency to die and undergo change in contact with oxygen, and so the vitalistic hypothesis made no progress. But no authority, however great, can prevent the truth from shining out in the end, when once the facts are fully known, and since the publication of Pasteur's researches on fermentation, and on the allied subject of "spontaneous generation," the vitalistic theory has formed the nucleus about which all research and speculation on the subject of the ferments has centred.

Pasteur, as I have said, was the great champion of the vitalistic hypothesis. According to him, it has to be admitted that the yeast plant assimilates part of the material presented to it, using this for its growth and to produce new individuals, and converts the rest into alcohol and carbonic acid gas. He did not consider these latter substances to be the outcome of a mere contact between the yeast cells and the sugar, but thought they were manufactured as it were by the yeast in the course of its vital processes, much as carbonic acid gas is produced by animals. Thus, to put it familiarly, we may say that, according to Pasteur, fermenting organisms eat fermentable matter, but that instead of assimilating the whole, or nearly the whole, of what they consume, as the higher animals are accustomed to do, they decompose the greater part into the characteristic products of the fermentation. In the case of alcoholic fermentation these products are alcohol, carbonic acid gas, and one or two other substances which are produced in very small quantities. This statement does not convey the whole of Pasteur's hypothesis; many important details are, from necessity, omitted. I must add that his opinions were not consistent in all respects with the more recent ideas of those who regard sugar simply as the food, or rather as a part of the food, of the yeast.

It would be impossible to give a full account of the experiments by which Pasteur established the connection between the organised ferment, the yeast, and the fermented substance, the sugar, but I may give some illustrations of the evidence he brought forward. He succeeded in showing, first, that it is possible to produce alcoholic fermentation in a solution of pure sugar containing small quantities of a few mineral salts, by sowing in it a mere trace of yeast; such a quantity as will adhere to the end of a needle being quite sufficient to ferment a large volume of the solution, because the yeast multiplies in this mixture of chemical substances just as it does in the more complex mixtures used in making wine, beer, and other alcoholic liquids. It is true that this experiment is not final. It does not disprove Liebig's hypothesis. It affords no proof that dead matter does not accompany the original germs of yeast, and gives no evidence that yeast cells do not die in the course of the fermentation. Still, the fact that a saccharine solution ferments readily and freely under the circumstances described severely strains the doctrine that it is not the living cells but the dead and putrefying cells that produce the fermentation.

Secondly, Pasteur discovered that just as the production of alcohol is associated with the presence of yeast, so other fermentations are associated with other definite organisms—one organism producing lactic acid, another the butyric acid of butter, a third the acetic acid of vinegar, and so on, when each is provided with its proper nourishing material: a fact which had not previously been recognised, and which does not square at all well with the idea that the common cause of all these varied yet well-defined phenomena lies in the mere dead matter which may be supposed to accompany that which is alive.

Thirdly, Pasteur showed that those fermentations which can be brought about by contact with air are prevented entirely, if the air be efficiently filtered, and if all living particles in the fermentable liquid be destroyed by the application of heat before the filtered air comes into contact with it. This discovery was made in the course of his investigations into the question of "spontaneous generation," a matter which has been settled only in comparatively modern times, and to the study of which Pasteur was driven, as it were, by the opinions he formed in the course of his researches on the ferments.

These last results seemed conclusive. No one could doubt any longer the general nature of the connection between the organisms and the fermentations. It was too much to ask us to believe that the latter depend on putrefactive changes, produced under the influence of the oxygen of the air in the remains of dead organisms, after it had been shown that killing the organisms stopped the fermentations, even in vessels exposed freely to filtered air, but that nevertheless fermentation could be restarted by introducing living organisms into the fermentable solution. Pasteur's evidence was so convincing, his victory so complete, that at the last even Liebig admitted the existence of a direct connection between the living organisms and the fermentations, though he suggested that the power of the former depended on vibrations among the molecules of the ferments.

But no victory is ever final. Even when Pasteur was establishing the vitalistic hypothesis firmly in its place, facts were known which could not be accounted for by that hypothesis, and others soon revealed themselves; and so it has come about that now, when Pasteur's labours have come to an end and he himself has joined the immortals, science is still seeking to penetrate the mystery of the ferments; whilst the turn things have taken would certainly delight his great antagonist if he could come back to us for a little. For though the vitalistic theory is still supreme, it affords, in its most recent form, ample scope for those chemical and physico-chemical speculations in which Liebig excelled and delighted.

The fact is that great theories based on intelligent observation seldom are wholly wrong, or, even in the most restricted sense, that is, in relation to the actual, established facts, wholly right. The phenomena studied by Pasteur and his colleagues did not sufficiently cover the field of knowledge on the subject, and therefore, whilst his conclusions lead us to truth, they do not give us the whole truth. Splendid as his achievement was, it carries us only one step onward. It leaves us, in fact, still face to face with a question:—These organisms which produce the various fermentations—How do they act? Are the substances produced under their influence the outcome of the processes of nutrition of the organisms, or are they brought into existence in some way by "contact actions," whatever that may mean? How, in short, do the organisms do their work?

Nearly a century ago there was discovered in barley a substance which can cause a solution of starch to undergo a kind of fermentation, producing from it a sugar; and long before the time of Pasteur this active substance had been isolated, in an impure state, and named diastase. Not very much later a similar substance, animal diastase, or ptyalin, was extracted from the saliva, and another ferment, pepsin, which decomposes albuminous substances, such as white of egg, was recognised in the gastric juices, whilst Liebig discovered a substance, called emulsin, which is capable of splitting up the active principle of the bitter almond, amygdalin, into sugar, essence of almonds, and a poison, prussic acid, which has occasionally been left in the essence with most disastrous consequences. Now, these substances, this diastase, this ptyalin, pepsin, emulsin, these and many others like them present the essential characters of ferments. That is to say, each of them induces changes in certain other substances, and the effect produced is, chemically speaking, out of all proportion to the amount of matter required to produce it, a single grain of the ferment invertase having been known, for example, to transform 100,000 grains of cane sugar into the substance called invert sugar and to remain almost as active afterwards as it was before the experiment was made. But none of these show any sign that they consist of organised bodies like yeast, *not one of them is capable of reproduction.* Thus the vitalistic hypothesis in its earlier form was insufficient. It took no count of the unorganised ferments.

Now, it has been observed that the unorganised ferments or enzymes are derived in every case, if not from fermentative organisms, at any rate from living cells, such as those which compose the ferments of Pasteur. Hence the question arises, may it not be that organised ferments like yeast secrete enzymes, and that these latter are the direct and immediate causes of fermentation?

This idea is not a very new one. It was put forward first by Moritz Traube in 1858, when Pasteur had only just begun his famous researches, but at first it seemed open to some objections. At that time no enzyme which was able to produce fermentation in the absence of the cells from which it sprang had been separated from any of the organised ferments, and thus Traube's ideas did not at once win general acceptance. But they had the great merit of bringing the two classes of ferments into correspondence, and they seemed to give a hint about the nature of some of the processes which go on in the living cells of organised structures, and hence they were not allowed to fall into oblivion. Later, as it became clear that the vitalistic theory in its earlier forms was unable to embrace all the facts, it began to be admitted more widely that, after all, fermentations might not result from the direct actions of the organisms, but might be due to enzymes generated by them, and that these might act inside the generating cells, or might escape from these cells and do their work outside them, as in the case of the ptyalin of the saliva. For a time, however, it seemed necessary, even if Traube's hypothesis were accepted, to suppose that in some cases the action is of another kind, because it was found impossible to isolate the enzymes corresponding to some important fermentations, including the most interesting of all, viz. zymase, the enzyme of yeast. But not very long ago (1896) Dr. Buchner succeeded in extracting from yeast this long-sought enzyme, and showed that it excites alcoholic fermentation in solutions of sugar as yeast itself does. This discovery, and others which support it, have greatly strengthened the position of this latest hypothesis, which now seems to cover the known facts, and appears likely to be generally adopted, if indeed this is not already the case.

But still, it will be noticed, we do not say how the enzymes act. We seem to have tracked fermentations to their source, first in the organisms, and then in the enzymes secreted by the organisms, only to find that the secret still evades us. What we have learnt leaves us asking, How do the enzymes cause fermentation? What is the quality which enables them to determine changes in relatively large masses of matter, and yet themselves remain almost unaffected at the end?

If we attempt to find a solution of our problem in this new form by comparing the phenomena of fermentation with the more familiar cases of chemical action, we find at once that the former in no way resemble such changes as combination or decomposition. For in these the acting substances disappear in quantities bearing constant relations to those of the substances produced from them. On the other hand, we do meet with chemical phenomena which resemble those brought about by the enzymes in a most striking manner. Thus we know that many substances will not interact chemically when they are perfectly dry. To enable sulphur or carbon to burn in oxygen, for example, a trace of moisture must be present; dry chlorine does not attack dry sodium; and again, dry ammonia does not combine with dry hydrochloric acid, nor will heat decompose the salt they form, chloride of ammonium, in the absence of moisture, though in every case only a trace of water need be present. Plenty of other facts of a similar kind might be quoted were it necessary to do so. Now, changes of this kind have been called "contact actions" or "catalytic actions," and they resemble the changes brought about by the enzymes in one notable respect, viz. that the quantity of the "catalyst" may be small, and sometimes exceedingly small, relatively to that of the substances in which it induces change. Hence it seems that the actions of the enzymes may be classed with the "contact actions" of chemistry; especially in view of the fact that the effects produced by some important enzymes, such, for example, as the transforming of starch into sugar, can be brought about also by means of purely inorganic substances, for instance, in the case quoted, by dilute sulphuric acid.

It is clear, then, that the actions of the enzymes may be labelled "contact actions." But, alas, this does not carry us far, and there is nothing quite so dangerous in science as a label, if it is wrong, or means nothing. Did not Liebig miss the discovery of iodine through labelling it chloride of bromine, and forthwith putting it away in his store room? And unfortunately in the case before us we really do not know what our label means. We do not understand the "contact actions" of inorganic chemistry; we cannot say why many chemical changes refuse to take place in the absence of water, or why dilute sulphuric acid generates sugar from starch in an aqueous solution. We know that water is taken up in this change, both when it is induced by dilute acid, and when it is brought about by the presence of the ptyalin of the saliva. But we do not know what part is played by the former substance whose composition we do know, and still less do we understand the action of the latter whose composition we do not know. All that we can conclude from the facts before us is this: that possibly the action of the enzymes is a chemical action, and akin to that of the catalysts of the chemist.

One more interesting fact has been discovered about the enzymes. From our studies of chemical change we know that if a solution of a compound of an element A with another element B acts upon a solution of a compound of C with D, so as to form new compounds containing A and D on the one hand and C and B on the other, then this change will not complete itself, that is to say the compounds AB and CD will not be converted completely into AD and CB, but some of AB and CD will remain unchanged, unless either AD or CB is thrown out of the sphere of action by reason of its falling as an insoluble precipitate or escaping as a gas as quickly as it is formed. Therefore, if the enzymes act chemically, we might expect them to be incapable of bringing about complete changes in certain cases:—to be incapable, let us say, of completely transforming the starch in a given solution of starch into sugar, since sugar is a soluble solid and therefore would neither be removed by precipitation nor escape from the solution as a gas, but would remain dissolved in the water. Now this is exactly what sometimes happens ; diastase, for example, cannot completely convert the starch in a solution into sugar unless the sugar be removed mechanically as it is formed.

Again, a ferment which can bring about one of these reversible changes might be expected, from experience of a purely chemical kind, to be able to induce the reversal of that change, and we have some reason to think that this also is the case. But this again, if finally established, would only show that the actions provoked by the enzymes obey the same laws as similar actions which fall definitely into the domain of chemistry. It would not tell us how the enzymes act, or whether they are themselves in any way modified by the results of their actions, and if not why not.

But though the mode of action of the enzymes thus remains hidden for the present, great progress has been made. Pasteur and his successors have traced fermentation to its source. They find this source in living cells or in special forms of matter elaborated by living cells. At first fermentative power was recognised as pertaining only to the lower forms of life—to yeast, to the vinegar plant, to the organisms which produce decay, and so on. But the discovery of the enzymes, and especially the fact that these are not secreted by the lower organisms alone, but, as in the case of ptyalin and of emulsin, also by the cells of the most highly organised animals and plants, makes it evident that what we call fermentative changes are not special phenomena which are peculiar to this or that group of low organisms, produce this or that useful product, or this or that deadly disease, but are manifestations of a general property possessed by living cells—that fermentative changes are the results of actions which go on, it may be, in every active cell in every living organism; in the cells of the simplest microscopic vegetable growth, and equally in those which compose the tissues of a Shakespeare or a Newton. And if this be so, have we not made a great stride forward and learnt something definite and important, if not about Life, "the organism in action," at any rate about the chemical phenomena which form part of the activities of the living cells of which every living being is composed?

ABOUT SOLUTIONS

WHAT happens when one dissolves a lump of sugar in a cup of cold water? Why does mere contact with water cause sugar and many solids, which require a good deal of heat to melt them, to run so easily into the liquid form? Do the water and sugar combine chemically, or is the change a physical change, like the melting of a solid when it is heated? And if the latter explanation be the true one, whence comes the energy required to overcome the attractive forces which hold together the molecules of solid sugar?

These questions, so interesting in themselves, have puzzled physicists and chemists for a long while. When we can answer them clearly and fully, so as to include in our explanation the dissolving of metals one in another, the separation, again, on cooling, of metals or combinations of metals from the solutions thus formed, and the processes by which the igneous rocks were formed in due order from the "rock-magmas" out of which they came in distant ages, we shall have created two new sciences—a new metallurgy and a new mineralogy.

It is nearly always profitable to study old and familiar subjects from a fresh point of view. Dalton gave a new lease of life to the atomic theory when he showed us how it could be used to explain the laws of chemical combination. Pasteur, the chemist, not only established the vitalistic theory of fermentation, but, through the influence of his work on other minds, paved the way for revolutions in medicine and surgery by his studies, at Lille, of the ancient art of brewing; and whilst the new chemistry owes an almost incalculable debt to modern physics, the organic chemists have placed data at the disposal of the physicists which make it possible to-day to attack the difficult problem of determining the shapes of atoms. And so it has been in regard to the phenomena of solution. As long as these were considered chiefly by chemists, from the chemical point of view and with the aid of hypotheses which assumed the existence of attractions—now of physical attractions, and then again of chemical attractions—between the solvent and the solute, as the dissolved substance is termed, comparatively little progress was made, though many interesting facts were discovered. But since the establishment of the kinetic theory of matter and the advance of chemistry have suggested new ways of attacking the problem, our knowledge has grown by leaps and bounds and other important branches of science have become involved until the theory of solutions, though still a matter for debate, has become almost a separate department of science.

As I have already said, it has long been a matter of debate whether the phenomena of solution fall into the domain of chemistry, or whether they belong to physics. If we try to understand what happened twenty centuries ago when Cleopatra dissolved a pearl in a cup of vinegar, and, again, what happens every time we dissolve a lump of sugar in a glass of water, two explanations at once suggest themselves. First, we may suppose that the sugar and the water combine, as oxygen and hydrogen combine when they form water, or as *caloric* was supposed, by Dr. Black, to combine with ice when the latter is melted. Secondly, we may suppose that particles, perhaps single molecules, of the sugar jump away from its surface and thus escape and mingle with the particles of the surrounding water, much as the molecules of one liquid are supposed to mix with those of another in the process known as liquid diffusion.

And it is easy to defend one hypothesis or the other by selecting suitably the cases on which we base our argument. Thus, when sugar is dissolved in water, there is but little evidence that they combine, or that any other chemical change occurs, and, moreover, the sugar may be recovered unchanged from the solution by merely allowing it to evaporate. To apply the chemical hypothesis to this case would appear somewhat gratuitous. On the other hand, the pearl dissolved in Cleopatra's fabled draught was destroyed for ever, converted, as any chemist would show you, into a white crystalline solid, called acetate of calcium, which is as readily soluble in water as a pearl in vinegar, and differs from a pearl in a dozen ways. Here it is manifest that a chemical explanation is involved, but the case is too complicated, we must not pursue it, but rest content for the moment to have gained a glimpse of two sides of this knotty question.

One of the pillars of the chemical theory of solution is to be found in the fact that many soluble substances combine readily with water, forming an important class of compounds known to chemists as "hydrates."[1] If, for example, you dissolve some dry copper sulphate, which is a nearly white solid, and some dry soda-ash in water, and evaporate the solutions to recover the two salts, you will obtain from the first not the original white solid, but magnificent blue crystals, identical with the "blue-stone" so often to be seen in the windows of the pharmacist; and from the second, handsome colourless crystals, not of soda-ash, but of the more familiar "washing-soda." If you weigh the two substances before you dissolve them, and collect and weigh all the crystals obtainable from their solutions, you will find in each case that these crystals not only differ from the original substances in many of their properties, but also largely exceed them in quantity; for from two pounds of the colourless copper salt you may obtain about three pounds of blue-stone, and from one pound of soda-ash almost three pounds of soda-crystals. Finally, if you distil the crystals you will get from each a great deal of water, and, by the exercise of proper skill, you may recover the salts you started with undiminished in quantity. It is quite clear, therefore, that copper sulphate and soda-ash form definite compounds with water. With these facts before us, what could be more reasonable than to suppose that copper sulphate and soda-ash combine with the water in which they dissolve? And if so, does it not seem likely that other salts also may form similar combinations when we dissolve them, even though we may have failed to isolate these compounds up to this moment? Now we have only to make this simple assumption, so easy to accept, and to extend it a little by supposing that in solutions we have to deal, not with solid hydrates like washing-soda and blue-stone, but with liquid compounds of a similar order, and we have the hydrate theory of aqueous solutions. Extend the idea a little by supposing, further, that other liquids act similarly with the solids they dissolve, and we have a theory of solution covering, if not the whole of our field, at least a very large part of it.

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[1] Hence it has come about that the chemical theory of solution is often labelled the "hydrate theory."

There was a time, not so very long past, when it almost seemed that the hydrate theory of aqueous solutions was securely founded. But there were always certain difficulties in the way of its final adoption. Thus, hot water, as a rule, is a better solvent than cold, but heat, on the other hand, decomposes hydrates readily. These two facts seem to clash. For how can heat, which drives water from salts, also promote the combination of salts with water? And then, again, as has been pointed out frequently, the fact that crystals of a hydrate, like washing-soda, come out of a solution, affords no clear proof that this compound ever existed in the solution. It is quite possible that the salt and the water may enter into combination at the moment when the crystals form, for phenomena of that kind are very familiar to chemists. Thus it is difficult to accept the hydrate theory unreservedly, in spite of the fact that its supporters have successfully applied it to the discovery of new compounds.

Nor can we, on the other hand, discard the hydrate theory altogether, and say that in no case is dissolving accompanied by chemical change. For even if it be admitted that in the simpler cases dissolving may be a purely physical phenomenon, yet there are many in which it seems clear that the more obvious, physical changes are accompanied by hidden chemical metamorphoses. It would be difficult, for example, to explain the chemical and electrical properties of a solution in water of the oxide of sulphur, called sulphuric oxide, upon the assumption that it consists of molecules of oxide of sulphur wandering uncombined among molecules of water. If, however, we start by assuming that the oxide reacts with some of the water, the properties of the solution at once become consistent with current electrical theory; whilst there are other solutions whose qualities distinctly suggest that in them at least we may really deal with hydrates like those formed by copper sulphate and soda-ash.

In order to prepare a true chemical compound —say water—one must combine its constituents in certain fixed proportions, namely, eight parts of oxygen with one part of hydrogen—no more and no less. If, when we bring together these gases, we take an excess of hydrogen or an excess of oxygen, the excess will remain unaltered when the combination is complete. Moreover, the proportions in which the gases combine are independent of the temperature at which they act. Now one of the first things the chemist discovers when he dissolves a solid in water is this: That the proportions in which the solid and the liquid will form a solution may be varied between very wide limits. When, for example, he dissolves sugar in water he can obtain a great variety of solutions of different degrees of concentration, which vary correspondingly in their properties, up to a certain limit at which the solution is said to be saturated, since it can then be made no stronger, and he finds further that this limit is not a fixture, but depends upon the temperature at which the solution is made. He can dissolve more sugar in hot water than in cold; and if he cools a hot saturated solution, crystals will be deposited and a weaker but still saturated solution will remain. Thus in this respect solutions of solids in water differ distinctly from the substances known as chemical compounds. This seems fatal to the chemical theory of solution till we remember that when nitric acid reacts with quicksilver somewhat similar complications occur, although in this case the action is unmistakably a chemical action. With such contradictory facts as these before us it is clear that for the present we can neither accept nor reject the hydration theory.

Let us now turn our attention to the rival hypothesis, and, to begin, let us try to picture what happens when a quantity of sugar is thrown into water, making the assumption that no combination occurs between the solvent and the solute.

According to the kinetic theory of matter, a liquid such as water, alcohol, or ether consists of a number of molecules so closely packed that they exert a certain amount of attraction on each other and as a whole hold together, but which, nevertheless, are so far free that they can readily slide over each other, each molecule being at liberty to wander among the rest, though it cannot move very quickly away from any given position, because, like a man in a crowd, it is constantly impeded by its neighbours. Now imagine that a layer of one of these liquids rests in a shallow dish, and that at a given moment a few hundreds or thousands of the molecules at the free surface of the liquid are moving, as would happen continually, away from the liquid and towards the air above it. Then, in spite of the surface tension of the liquid, which tends to retard the progress of these molecules, some of them will escape from the liquid and pass into the less closely packed air ; and though, owing to subsequent collisions with air molecules, some of the escaping molecules would afterwards return to the liquid, many would not return, and these would constitute what is called the vapour of the liquid. Here we have a picture of the process by which every volatile liquid slowly or quickly "dries up" or evaporates when freely exposed to the air. If the liquid be exposed to a limited volume of air, or placed inside a small vacuous chamber, evaporation still occurs, but to a much more limited extent, as the following passage will show. If you make a rough mercurial barometer by filling a glass tube a little above forty inches in length, and closed at one end, with quicksilver and inverting it in a basin filled with the same liquid, and then introduce a few drops of a volatile liquid into the tube, so that they may rise and form a layer in the vacuous space above the quicksilver, you will find that the mercury falls to a greater or less extent, according to the liquid you employ and the temperature at which you make the experiment. If water is used, and if the temperature be 20° C., the mercury will fall about 17 mm., whilst at 100° C. the water vapour will drive the quicksilver down the tube until it stands no higher inside the barometer than in the basin. The quantity of water used in no way influences the result of the experiment, provided that sufficient be present to keep the surface of the mercury wet. If ether is used instead of water, then at 20° C. the mercury will not fall 17 mm. but about 430 mm., and the mercury will be driven to the bottom of the tube at a proportionately low temperature. Thus we see that evaporation produces pressure, that this pressure can be measured, and that its amount depends on the nature of the liquid and on its temperature.

Now I think it will be evident to every reader that, upon the above theory of evaporation, we can account for this pressure in the following way. Directly the liquid rises above the quicksilver in the barometer, molecules escape from its surface and fly off into the space above it. Thus very soon this space contains vast numbers of molecules, which move like the molecules of a gas in every direction with great velocities, in straight lines, and dash against one another, against the sides and the end of the tube, and against the surface of the liquid, not in successive volleys like bullets in volley firing, but in what would seem, if we could see or feel them, never-ending, never-slackening universal streams. Every impact of a flying molecule on the glass and on the quicksilver produces pressure, the amount of this pressure depending partly on the mass of the molecule and partly on the velocity with which it moves. And it is the collective pressure of the molecules falling at any moment upon the movable column of quicksilver which we detect and measure, in the experiment just described, by noting the extent to which the quicksilver is driven down the tube. Since the space at the top of a barometer is very limited, it is easy to see that under the conditions of our experiment, molecules which have escaped from the liquid must soon begin to return to it, that the more numerous the free molecules become the more frequently they will return, and that a point must soon be reached at which molecules escape from and return to the liquid at equal rates. When this state of equilibrium is reached, the pressure due to the bombarding molecules becomes steady so that we can measure it. We can increase this pressure by warming the liquid, for by so doing we impart additional energy to the molecules. This makes them move more quickly than before, and causes them again to escape from the liquid more rapidly than they return. But this new state of affairs does not last long ; soon the rates of escape and return again come into equilibrium. Then the pressure once more becomes constant, and remains so as long as the temperature of the liquid neither rises nor falls.

Let us return to solids and solutions. Many solid substances, *e.g.* camphor, are volatile like water, and many others not so obviously volatile as camphor exhibit properties which suggest that they also are liable to lose weight by the escape of molecules from their surfaces. Thus ice and snow gradually disappear even in the coldest weather, and have, in fact, a minute but measurable vapour pressure; many solids are odorous and, therefore, must give off particles of matter, and, as Sir Roberts Austin showed us a few years ago, if a piece of gold is kept in close contact with a piece of lead for a few years, particles of the gold pass into the lead. It is clear, therefore, that many solids possess a property analogous to that by which we account for the vapour pressure of liquids, and that we may carry over to the case of a solid immersed in a liquid the notions we have gained by thinking of the properties of liquids. If we do this, if we picture to ourselves a lump of sugar immersed in a cup of water and assume that molecules can escape from the surface of sugar as they do from the surface of a drop of water, we see at once that the sugar may be regarded as the analogue of the water and the water as the analogue of the air, or of the vacuous space, in the experiment on vapour pressure described above. Now suppose that molecules of sugar escape continuously from the surface of the sugar, just as molecules escape from a mass of ice when it evaporates during a frost, then these escaping sugar molecules will pass into the water and mingle with it. The first swarm of sugar molecules will be followed without any interval by a second, the second by a third, and so again and again and again without cessation, and thus the number of molecules of sugar in each cubic inch of the solution will increase every moment. Before long some of these wandering molecules of sugar, in the course of their travels among the water molecules, will come again into contact with the sugar and be retained. At first, as in the evaporating of a liquid, the escaping molecules will be many and the returning molecules few. But, as every addition to the number of sugar molecules in a given volume of the solution involves an increased rate of return, while the rate at which they depart is constant,[1] sooner or later this give-and-take process must reach a stage at which the departing and returning molecules balance one another. Then the solution will grow no stronger, and we shall have a "saturated solution."

If we raise the temperature of the sugar when this stage is reached, we shall cause the sugar molecules to move more quickly and fly off into the water more rapidly than before, and the concentration of the solution will increase. But this, in the end, hastens the rate at which the molecules return also, and thus a new state of equilibrium corresponding to the higher temperature is quickly reached, and again we have a saturated solution, which can only be made stronger by raising the temperature to a still higher level. On the other hand, if we cool the syrup we reverse the whole process and remove sugar from the solution. This is just what we do when we prepare sugar crystals by cooling a hot concentrated syrup.

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[1] Or would be if the area of the surface of the sugar did not diminish.

Here, then, we have a picture of the mechanics of solution which accounts for the phenomena without assuming the existence of attractions of any kind between the particles of the solvent and those of the solute. This theory, or model, is a purely physical theory and it rests on the kinetic theory of matter. It does not, it is true, cover the whole of the ground; there are cases of dissolving in which it is difficult to deny that chemical action occurs, and our theory does not explain why some solids are soluble and some insoluble, or why water is the most general solvent for salts, and alcohol and allied liquids more useful in the case of compounds of carbon. Nevertheless it helps us forward. It is a good beginning.

If you accept the above conception of solutions and regard them as homogeneous mixtures in which the molecules of each constituent retain their individuality and move freely among one another, and if, further, for a moment, you disregard the solvent and think only of the molecules of the solute, you will perceive that in very dilute solutions, where the molecules of water are many and the molecules of the solute few, the conditions of the latter exhibit a remarkable analogy to those of the molecules of a gas, as described in the essay on "Matter, Motion, and Molecules." For in a dilute solution, as in a gas, the molecules of solute present in a given volume are comparatively few in number, for the most part widely separated, and hence out of the range of their mutual attractions and only liable to encounter one another as they move about in the liquid at comparatively long intervals. This suggests that a close scrutiny might reveal analogies between those properties of dilute solutions which are due solely to the solute and the properties of a true gas. Now such analogies do exist. They were first pointed out by Professor van 't Hoff, whose conclusions were founded on the results of some studies of "osmosis" made by Pfeffer, the physiologist, about thirty years ago.

If one attaches a small parchment bag to one end of a narrow glass tube, fills the apparatus with alcohol, or with a solution of sugar in water, so as to make an instrument very like a thermometer with a parchment bulb, and then immerses the parchment bag in a vessel of pure water, one soon sees the liquid rise in the glass tube like the quicksilver in a thermometer when it is heated. This is due to the fact that water can pass through the parchment and enter the bulb more quickly than sugar or alcohol can escape from it. Now the passage of liquids through parchment, bladder, and other similar tissues, which is called "osmosis," has been studied and re-studied repeatedly on account of its importance in physiology ever since it was discovered by the Abbé Nollet in 1748; but owing to the circumstance that parchment and bladder are not wholly impermeable to solids like sugar, the importance of the phenomenon in physics was overlooked till a few years ago, when membranes were prepared which were strong enough to bear very considerable pressures, and truly impermeable to these latter substances though freely permeable to water.

When these "semi-permeable" membranes became available, effects like those described above were made the subject of exact study; and presently it was discovered by van 't Hoff, from measurements previously made by Pfeffer, that the pressures called "osmotic pressures," which are produced inside vessels formed from these membranes when they are filled with solutions of various solids and immersed in pure water, depend on the nature of the substances in solution, and might be used for determining the concentrations of the solutions. And, further, that just as we can deduce the relative weights of the molecules of gases from measurements of the relative densities of these gases made at known temperatures and *pressures*, so we can deduce the molecular weights of many substances in dilute solutions, if we measure the concentrations of these solutions, their temperatures, and their *osmotic pressures*. Or, to put the matter in another way, that in many cases the quantities of substances which exert equal osmotic pressures correspond to their relative molecular weights, and, therefore, might be employed to measure the molecular weights of the substances. Measurements of osmotic pressures have not, it is true, supplanted the earlier methods of weighing molecules; but the above facts and the circumstance that osmotic pressures vary with changes of temperature in a manner which corresponds with the behaviour of gases forces us to ask ourselves the question, May we not transfer to the molecules in dilute solutions what we know about the molecules of gases? If so, must we not suppose that the former, like the latter, are free and without attraction for one another? Must we not, that is to say, regard a dilute solution as a mere homogeneous mixture of its constituents comparable with such a mixture as atmospheric air? How are we to reconcile such an hypothesis as this with the facts of a different order which may be brought to the support of the hydration theory?

We must now turn to another side of our subject. As long ago as the year 1857, Clausius, the founder of the kinetic theory, suggested an explanation of electrolysis which assumed first that every electrolyte consists of two parts, identical, in the case of salts, with the metallic and acid radicles whose exchanges can be traced in chemical reactions. Secondly, that when a salt—say common salt or sodium chloride—is dissolved in water, the atoms of chlorine and the atoms of sodium which make up the molecules of the salt do not remain permanently coupled, but change partners from time to time, and occasionally even lose their partners, as the result of encounters between the molecules, and thereafter wander alone in the solution until they collide with atoms of the opposite kind, when they may again combine and reconstitute molecules of sodium chloride. And, thirdly, that when a compound is, as we say, decomposed under the influence of an electric current, the free atoms, or "ions," alluded to above, form the vehicle by which electricity is carried between the electrodes. On this view of the nature of electrolytes an aqueous solution of sodium chloride would contain, at any moment, not only many molecules of the original salt, but also a certain number of free ions.[1] These latter would go to the two electrodes in the process of electrolysis, and fresh supplies would constantly be produced by the continuous dissociation of further portions of the salt, until the whole had been resolved into its constituents.

We have seen that in 1887 the great Dutch chemist, van 't Hoff, pointed out that if we prepare dilute solutions of two substances A and B, whose molecular weights are already known, making these solutions of such concentrations that equal volumes—say one pint of each—contain equal numbers of molecules, and compare their osmotic pressures, we shall find these pressures are equal. There are, however, some notable exceptions to this rule, and it is found that when a solution, which conducts electricity, for example, a solution of common salt, is compared with a solution which does not conduct electricity, the osmotic pressure of the former is considerably greater than that of the latter. Now we might explain this by supposing that van 't Hoff was wrong, or we might explain it by supposing that mistakes have been made in determining the molecular weights of the substances used in our experiments. But there is at least one other reasonable alternative. Suppose that the Clausius theory of electrolysis is true, suppose that the molecules of electrolytes in solutions are partly broken up into ions, and not only to a slight extent, as Clausius imagined, but to a considerably greater extent than he suggested. Then there would be a greater number of particles present in the given volume of the conducting solution than in a corresponding volume of the non-conducting solution, and the actual osmotic pressure of the former would be greater than its calculated osmotic pressure. For the dissociation of the molecules of the salt into their ions, by increasing the number of particles present in a given volume of the solution, would raise the value of those of its properties which, like osmotic pressure, depend upon the number of particles present in unit volume of the liquid. If we accept this solution of the problem, and it is supported by other qualities exhibited by solutions of electrolytes, then it would seem that the abnormal osmotic pressures of substances of this class are not the result of error, but, on the contrary, vindicate the electrolytic dissociation hypothesis, and afford us a means of measuring the extent to which dissociation has taken place in any given solution of an electrolyte.

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[1] See p. 178.

But beautiful and attractive as these last speculations may seem, we must not forget that, after all, the conditions of the molecules of a solute as they wander for the most part in solitude and far apart from each other amid relatively dense swarms of the molecules of the solvent, may not be strictly comparable with the conditions of the molecules in a gas, for these are not thus hampered by closely packed crowds of attendant alien particles. I ought to add, therefore, that some of the ablest of those who have studied solutions are disposed to hold that the chemical theory may yet be so extended as to bring it into harmony with the facts which at present stand in the way of its acceptance ; while others, still fascinated by the physical aspects of the problem, are disposed to transfer their attention from the solute to the solvent, and suspect that "the active rôle in aqueous solutions" is played not by the substance dissolved, but by the molecules of the water in which it is dissolved.

"An ideally perfect solution," said the President of Section B, at the Cape meeting of the British Association, "that is, a solution the physical properties of which are determined solely by the number of molecules it contains in a given volume, must consist of a solvent and a solute which have no chemical affinity for each other, so that their molecules will neither associate nor dissociate in solution."

If we could accept this view of the matter the problem would become relatively simple, and its solution might be near at hand, for then we should be justified in confining our attention to a few of the less complex cases which, it must be admitted, is pretty much what has been done in this present essay. But the whole truth about solutions will hardly be reached by following this narrow road. When we study phenomena connected with solutions, whether it be the liquefaction of a solid like sugar or salt in water or some other familiar liquid, the dissolving of a solid metal in a melted metal, the dissolving of a natural silicate in a molten rock, or reversals of these processes such as occur in the crystallising of salts from their solutions, of metals or metallic complexes from molten alloys, or of the naturally occurring minerals from rock-magmas in prehistoric times, we are required, as two great modern workers in this field have remarked, to read a true palimpsest. In every one of these cases Nature has inscribed, as it were, two stories on a single parchment. There is the physical inscription, there is the chemical inscription, closely written, one upon the other, in every direction all over the parchment. We may seek to decipher one or we may seek to decipher the other, but neither by itself will yield the whole truth. If we want that, sooner or later we must read them both.

THE ORIGIN OF LIFE

“The modern spirit is not the spirit which always denies, delighting only in destruction. Still less is it that which builds castles in the air rather than not construct.”

CAN life arise in these days and under the conditions which now exist without the agency of pre-existing living matter? Is life biogenic or abiogenic in its mode of origin?

What has experimental science to say on this question?

For centuries upon centuries nearly every one who has thought upon this subject appears to have come to the same conclusion. Poets and philosophers, the learned and the unlearned, all alike have answered the question which stands at the beginning of this essay in the affirmative. Doubt seems to have been to them incomprehensible, and thus the words of St. Paul: “Thou fool, that which thou sowest is not quickened, except it die,” expressed equally well the conviction of the most learned of the philosophers and that of the humblest of the disciples of Christ. Nor, as far as one can judge, did the progress of time weaken this widespread conviction, for we find such leaders of thought in the Middle Ages as Bacon and Van Helmont[1] making, on the subject of spontaneous generation, some of the strangest assertions to be found anywhere in the annals of science. Thus, in the "New Atlantis" (1627) one of the fathers of Solomon's house declares that in the enclosures and parks of that wonderful palace of research, we find means to "make a number of kinds of serpents, worms, flies, fishes of putrefaction, whereof some are advanced (in effect) to be perfect creatures like beasts and birds; and have sexes and do propagate. Neither do we this by chance, but we know beforehand of what matter and commixture, what kind of those creatures will arise." "We have also means to make diverse plants rise by mixtures of earths without seeds; and likewise to make diverse new plants differing from the vulgar, and to make one tree or plant turn into another." Whilst Van Helmont (1577–1644),[2] the chemist, physician, and mystic, who saw his own soul "under the figure of a resplendent crystal" in the year 1633, and whose name will remind many of my readers of one of the most beautiful passages in that remarkable book "John Inglesant," actually gave recipes for the production of mice and vipers; saying that to produce a pot of mice "it suffices to press a dirty sheet into the mouth of a vessel containing a little corn, when, after about twenty-one days, the ferment proceeding from the linen modified by the odour[1] of the corn effects the transformation of the wheat into mice." Van Helmont even stated that he had himself witnessed this transformation, and that the mice were born full grown and of both sexes. Thus, during century after century, it was held by all that "the corruption of one thing is the life of another," and that living matter springs spontaneously from non-living matter, incessantly and on a vast scale in every direction around us. And all this time, as far as we can judge, no serious attempt was made to test the truth of these statements by making a really carefully considered experiment. But by the middle of the seventeenth century a change was at hand, and about the year 1670,

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[1] Van Helmont, though a mystic, was by no means incapable of devising and performing sound experiments, and both chemistry and medicine are indebted to him for important discoveries and reforms.

[2] His writings were not published till after his death, viz. 1648.

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[1] Van Helmont, who was a careful student of the gases, believed that smells play a great part in the generation of life. Thus he wrote: "The smells which arise from the bottom of morasses produce frogs, slugs, leeches, grasses, and other things."

Francesco Redi, the great Italian, a scholar and physician, the contemporary but rather the senior of Harvey, taking in his hand a few pieces of flesh and some strips of fine gauze, devised an experiment which marks an epoch in the history of human thought, and has formed the model for every experiment made on the subject of "spontaneous generation" from that time till to-day.

It is the fortune, as my friend Mr. Punnett has lately reminded us in the opening chapter of his charming little book on "Mendelism," of some great ideas to be received at once with the good will and acclamation of all. But it is the fate of others to win acceptance only after long and even bitter struggles and much conflict. Redi's doctrine *Omne vivum ex vivo,* or, as we should express it to-day, "no life without antecedent life," belongs to the latter class. From the latter half of the sixteenth century till to-day, every one who has investigated the origin of life has followed the lines laid down by the great Italian. But his conclusions have been contested again and again, and even at this moment, are the subject of a certain amount of suspicion on the part of many thoughtful people who, owing to want of familiarity with the details of the great discussions which took place a generation ago, were not able to separate for themselves the wheat from the chaff when reading the sensational articles on the origin of life which were so conspicuous in newspapers and magazines not very long ago.

Flesh, as we all know, when kept in contact with atmospheric air becomes putrid, and soon swarms with maggots. And the question is, do these maggots find their origin in the putrefaction of the flesh? Van Helmont and those who came before him answered this question in the affirmative. Redi's reply was—Let us make an experiment and find out. May it not be that these maggots come from some source outside the flesh on which we find them? Accordingly, the great Italian placed pieces of flesh in jars covered with fine gauze, which prevented the access of flies. When he did this, the flesh became putrid as before; but the most careful observer could detect no maggots upon it, though other pieces of the same flesh placed beside it, but not similarly protected, soon swarmed with maggots, which are the larvæ of flies, in the usual manner. Is it not plain, said Redi, that the maggots we find on putrid flesh are not created by the putrefaction of the flesh nor by the action of the air or of any vapour, both of which could find their way through the gauze, but are produced from eggs laid by the flies? If any confirmation of this conclusion is needed, it is only necessary to let meat putrefy inside a gauze cage, when blow-flies, attracted by the odour, will visit the vessel and, since they cannot reach the flesh, lay on the muslin eggs from which maggots are quickly hatched. Redi's experiment, as you see, was almost childish in its simplicity and directness. It went straight to the heart of the matter. Any one can repeat it for himself. Every one who repeats it confirms Redi's conclusions, if he really keeps the flies away from the putrefying matter.

It would be impossible within the limits of this brief essay even to sketch in the outlines of the whole history of this great experiment. Therefore, I must ask my readers to be content with a glance at two or three of its most important stages. The first of these resulted from the improvement of the microscope. The second was the outcome of the discovery of oxygen and of the oxygen theory of fermentation. The third and last remains still in embryo, and seems exceedingly unlikely to pass beyond that stage.

Redi's experiment dealt quite conclusively with the facts known to biologists in the seventeenth century. It accounted for all that can be seen by the naked eye. But the improvement of the microscope soon modified the problem by revealing the fact that flesh and other organic matter in a state of decay or fermentation is not, as Redi supposed, truly dead, but swarms with minute organisms of various kinds. This, of course, reopened the whole question, raised grave doubt as to the generality of Redi's conclusions, and led Father Needham, in the eighteenth century, to put the matter again to the test by making fresh experiments. Let us assume, said this naturalist, that Redi is right, that there is no life without antecedent life, and that these microscopic organisms which we find in all decaying matter are produced from eggs or germs. Then, since all the eggs or germs known to us can be deprived of their vitality by heat, it follows that if we apply heat to flesh or vegetable matter, we shall kill the germs, and the matter will become incapable of putrefaction, or, if it still decays, will produce none of these organisms which the microscope reveals.

Acting on this idea, Needham and others repeated Redi's classical experiment, using aqueous infusions in place of lumps of flesh, protecting these infusions from external sources of contamination far more carefully than their great predecessor, and heating them strongly before studying their behaviour. The results of these new experiments were contradictory. In the hands of Needham they did not support Redi's conclusions, for Needham found that however carefully he made his experiments the infusions decayed, and minute organisms made their appearance sooner or later in every case. In short, Redi's hypothesis seemed to break down. Presently, however, the Abbé Spallanzani showed that Needham's experiments, though admirably conceived, had been ineffectively performed, and that organic infusions in no case produce animalcules if the vessels containing them are *sealed hermetically*—*i.e.* by melting their necks before the blow-pipe—and heated for at least forty-five minutes to the temperature of boiling water.

Spallanzani's research was a great victory for Italian science. It not only reasserted the validity of Redi's method, but showed how his experiment may be modified to meet the new conditions which had resulted from a century of progress. Moreover, Spallanzani's results gave the death-blow to the ingenious theory of Buffon, which at one time had a considerable vogue, that life is the peculiar characteristic of certain organic particles or groups; that the higher organisms of every grade are produced by the combining of these "organic molecules," as they were called; that what we term death and decay consist in the breaking up of these temporary associations of organic particles, and that these particles, when they are liberated after the death of the organism, constitute the minute animalcules which the microscope invariably detects in decomposing infusions. Thus under the influence of Spallanzani the theory of biogenesis regained its sway, but only to be challenged once more when the progress of chemistry had revealed the importance of oxygen in relation to life, and shown its connection with the various fermentations and putrefactions.

It is characteristic of "ferments," such as yeast and the vinegar plant, that a little of one of them goes a very long way. In the case, for example, of alcoholic fermentation a single yeast cell, too small to be recognised by the naked eye, may be so used that it will suffice to convert cask upon cask of grape juice into wine; whilst, similarly, in the presence of air in sufficient quantities, a few almost invisible specks of the vinegar plant may be so applied as to transform all these casks of wine into vinegar. Now this significant fact was at first very difficult to understand, and the chemists, who came earliest into this field, endeavoured to explain it by an hypothesis suggested by the theory of catalysis. According to this hypothesis, which was strongly supported by Liebig, fermentations are a kind of "contact action," a morsel of the yeast or of the products of its decay being able to decompose particle after particle of sugar into alcohol and carbonic acid gas by mere contact, by a process similar to that by which black oxide of manganese causes chlorate of potash to give up its oxygen at a lower temperature than is required for decomposing the salt by heat alone.[1] Liebig considered that the processes by which animal and vegetable matter, from the smallest twig or insect to the largest oak or the remains of an elephant, are resolved into the simple inorganic compounds, carbonic acid gas, water, and ammonia, so that they become ready to nourish fresh generations of plants and animals, require, in order that they may occur, moisture and, at any rate at the initiatory stages, air. His idea being that "by the contact of these organic compounds with the oxygen of the air, a process begins in the course of which their constituents suffer a total change in their properties," so that after this contact "the state of rest or equilibrium of the attractions which keeps the elements together has been disturbed in a particle of the substance, and, as a consequence of this disturbance, a separation or new arrangement of the elements has been brought about." "The continuance of these processes, even when the oxygen, the original exciting cause of them, no longer acts, showing most clearly," as he believed, "that the state of decomposition which has been produced among the elements of a particle of the mass exerted an influence on the other particles which have not been in contact with the oxygen of the air; for not only the first particles, but, by degrees, all the rest undergo *the same change.*"

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[1] I should mention that our knowledge of the nature of contact actions is very imperfect.

It is not possible to produce putrefaction in the case of all organic substances. Sugar and starch, for example, may be exposed to the air with impunity so long as no "putrescible" matter is in contact with them. Therefore, Liebig divided organic substances into two classes—namely, those which, like sugar and starch, are subject to fermentation only when under the influence of a putrescible substance, and those which are themselves putrescible and the cause of fermentation in the other class, such as white of egg, flesh, and the gluten of wheat. This was Liebig's chemical theory of fermentation, in which, as will be observed, the oxygen of the air played a leading part. This theory fitted in well with the fact that one usually finds in the juices of plants and in the fluids prepared by macerating flesh in water, plentiful supplies of substances belonging to both these classes.

Liebig was of opinion that the power of the ferments, or putrescent bodies, over those which are only fermentescible depends on a state or condition of the atoms of the former. He supposed that a ferment was able to disturb the equilibrium among the atoms of the molecules of such a substance as sugar, in virtue of the motions of the atoms of its own molecules; and, according to his teaching, it is the function of the oxygen of the air to start the internal movements which change a putrescible substance into one that is actually putrescent, *i.e.* into a ferment. Hence, on his view, the immediate cause of putrefaction and fermentation is the chemical action of the oxygen of the air. That is why exposure to air is a necessary preliminary part of a fermentive change. I should add that in Liebig's opinion fermentation was only the first stage in the process by which complex organic substances are resolved into carbonic acid gas, water, and ammonia ; he held that the process as a whole was completed by decay, which he defined as "a process of combustion taking place at common temperatures, in which the products of the fermentation and putrefaction of plants and animal bodies combine gradually with the oxygen of the air." But ingenious and even convincing as this theory seemed at one time, it did not resist for long the blows dealt by its opponents, and especially by Pasteur, the greatest among the recent upholders of the vitalistic hypothesis.

In 1680 Leeuwenhoek, when examining beer yeast with a microscope, observed the curious fact that it consists of small globules, and about a century and a half later Cagniard de la Tour, and afterwards others, discovered that these globules reproduce themselves by forming buds. This led them to conclude that yeast is not a mere chemical compound or mixture of chemical compounds, as till then had been supposed, but a vegetable organism, and that its action on alcohol results from the life processes of this organism. These observations and speculations may be regarded as the germ of the vitalistic theory as expounded by Pasteur in the nineteenth century.

As the result of a careful study of many separate fermentations, Pasteur succeeded in showing in a number of cases, that each fermentation, the alcoholic fermentation by which we produce wine, the acetous fermentation by which we produce vinegar, the lactic fermentation which gives lactic acid and so on, is so invariably associated with a particular organism that we find ourselves compelled to admit the existence of a connection between the organism and the fermentation. We cannot, it is true, prove that when a single yeast cell or other fermentive organism is sown in a suitable medium perfectly sterilised by heat, it does not carry with it a certain amount of decaying matter, and, therefore, we cannot disprove, by Pasteur's method, the contention of Liebig that "putrescible matter" in contact with air is the true source of every fermentation. But as "putrescible matter" alone, if sterilised, even when in contact with air (provided the latter is free from suspended particles), has never been shown to produce a fermentation in a perfectly sterilised fluid, this objection carries no weight. Consider the facts. If you place a few yeast cells together, perhaps, with certain non-living impurities which are attached to them, in a suitable saccharine solution, previously sterilised by heat, fermentation sets in, the yeast cells multiply and alcohol is produced; sterilise, *i.e.* kill, the yeast cells before you introduce them into the solution, and you get no fermentation and no alcohol. Can any one seriously argue, in the face of such facts as these, that it is the accompanying impurity, and not the yeast cells, which causes the fermentation? All the evidence which has been accumulated on the subject of fermentations supports Pasteur's contention, that where there is no organism there is no fermentation, and that each fermentation is associated with a particular organism, and can be produced at will by the introduction of that organism into a suitable nutrient substance. Not only do laboratory experiments support this view, but many important departments of human activity, such as brewing, vinegar making, and much of modern medical science are found to confirm the victory won, first, in the laboratory.

I must not omit to mention the fact that there are changes in many ways resembling fermentations which take place in the absence of living organisms, and that it remains to be proved whether the organised ferments act directly on the matter fermented, or indirectly by producing "enzymes." This subject has been more fully discussed in a previous article on "Ferments and Fermentations."

We must now return to our main thesis. The recognition of the fact that each fermentation is associated in some way with the life processes of a particular organism did not, it is true, afford an answer to the question—is life Biogenic or Abiogenic in its mode of origin? But Pasteur's researches made the "Origin of Life" more interesting than ever. The subject was no longer merely academic. It had become a matter of practical importance, for the health and general well-being of man might, and, as we believe, do depend upon the soundness of our knowledge of the origin and functions of the lower organisms. No one realised this more thoroughly than Pasteur. "He could not," said his biographer, M. V. Radot, who wrote under his direct inspiration, "proceed further with his researches on fermentations without first settling the question of 'spontaneous generation.'"

Pasteur's splendid contributions to the last great discussion on this subject appear to have owed their origin largely to the feeling mentioned above, which was stimulated perhaps by certain propositions made in 1860 by the Academy of

Sciences, and by a lecture delivered in 1858 by a French naturalist, M. Pouchet, a supporter of the theory of abiogenesis, who, like Needham, stated the problem to be solved with intelligence and precision, but in carrying out his experiment left just one opening by which dust from the air gained admittance to the sterilised contents of his apparatus. This rendered his experiments inconclusive, and, therefore, valueless.

Pouchet's conclusions were quickly overthrown by Pasteur, who repeated the work of Spallanzani with many refinements and variations, and not only confirmed the results of his distinguished predecessor, but, going further, showed that really sterile organic fluids may be exposed to direct contact with the air for hours, and indeed for days, at a time, provided that the air is pure germ-free air, such as that which blows over a glacier, or has been deprived of all suspended living particles by means of heat, or by filtration through cotton wool. He clinched the matter by examining the plugs of cotton wool, which had been used as air filters in his experiments, under the microscope. This revealed in them an abundance of minute organisms and their seeds or germs, and Pasteur conclusively established the character of these organisms by sowing them in the infusions he had previously sterilised by boiling; whereupon these infusions became even more productive of life than others simultaneously exposed to unfiltered air. It is interesting to add that at this stage Pasteur derived not a little support from Tyndall, who confirmed the great Frenchman's conclusions by showing that ordinary air is, as Huxley has expressed it, "a sort of stirabout of minute solid particles which can be largely destroyed by heat," and that these particles can be so completely removed by filtering the air through cotton wool, after the manner of Pasteur, that the air when so treated becomes optically pure—that is, invisible when a ray from a powerful lantern is passed through it.[1] A few years later, about 1877, Tyndall knocked in the last nail and removed the last doubt in the course of his well-known discussion with Dr. Charlton Bastian, by showing that though in the case of certain organic solutions a single heating does not always produce sterility, yet this can be absolutely secured by heating, cooling and reheating the liquid several times at suitable intervals. This precaution being necessary to ensure the destruction of the spores of the organisms, as these are not destroyed when heated to temperatures which easily kill the parent bacilli; a fact which was long overlooked, and led to not a little heated discussion, before Tyndall showed that this source of error could be removed by heating the liquid to be sterilised so as to destroy the fully developed organisms, allowing it to cool and remain for some time at a suitable temperature in order that the spores might in their turn develop into organisms, and then destroying the latter before they became mature and produced fresh spores.

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[1] If you recall the effect of passing a beam of sunlight into the air of a darkened room, the above will become intelligible.

If, now, we sum up what has been discovered since Redi made his famous experiment in 1670, we find: first, that if vegetable or animal infusions which have been completely devitalised by heat are exposed to atmospheric air, which also has been devitalised by heat, or carefully filtered through layers of cotton wool to remove from it every particle of solid matter, the air will produce no signs of life in the fluid, even after prolonged exposure. But that the same infusions on the admission of ordinary dust-laden air, or of fluids containing living organisms, rapidly produce abundant crops of living things. Secondly, that in numbers of cases the minute organisms which are associated with specific fermentations can be distinguished from one another in various ways, as, for example, by their appearance alone or in colonies when seen under the microscope, by the substances they produce when they are cultivated in suitable media,[1] by their physiological effects, and sometimes, again, by their colour. This, in brief, is the case of those who support the theory of biogenesis. It consists of a mass of facts very varied in their character which hold well together. On the other hand, it has to be admitted that from time to time organic fluids contained in flasks supposed to be hermetically sealed have exhibited signs of life after exposure to high temperatures for long periods. Concerning these cases it need only be said, first, that it is obviously unlikely that no one would ever fail in making experiments of this kind. Secondly, that in every great discussion, it has been shown, in the end, that the fluids used owed their fertility either to ineffective sterilising or to the admission of living matter through some unsuspected defect in the details of the experiment. At every stage the result has been the same. The experiments of Needham were shown to be untrustworthy by the more exact experiments of Spallanzani. The experiment of Pouchet, which for a moment

[1] For example, the yeast plant produces alcohol by fermenting sugar; the vinegar plant produces acetic acid in dilute alcohol. But these two do not exchange their special functions.

seemed to reverse all previous decisions, was quickly shown to be defective by Pasteur; and, in the great dispute between Tyndall and Charlton Bastian thirty years ago, the latter fell before his distinguished opponent, because Tyndall was able, as Pasteur once expressed it, to show "where the mice got in," in the experiments on which Dr. Bastian relied. Finally, Dr. Tyndall disposed of the most serious cause of failure by his brilliant experiments in 1876 and 1877, when he introduced the method of intermittent sterilisation to which I have alluded on a previous page. Since those researches were published failures have become exceedingly rare except in the hands of beginners at this kind of work.

But if we accept the decision that the results of experiments justify us in concluding that no living organism arises in these days except as the descendent of a pre-existing organism, we must not, as Huxley reminded a previous generation, transform this decision into a dogma, or presume to assert definitely that no process of spontaneous generation of life has ever occurred in the past history of our earth, or that no such process ever will take place in the future. All we are justified in saying is this: that we have no solid grounds for believing that living matter has been produced from non-living matter in the laboratory up to this day.

And now, in conclusion, let us turn to the newspaper chronicles in which it was proclaimed not very long ago, as from the housetops, that science, or rather "a scientist," if the expression may be excused, had at last discovered the origin of life, and inquire what all this pother was about, and what is the real significance of the experiments, which most of us have read or heard about, with radium and sterilised bouillon.

Those of my readers who have followed me so far, and who have read the details of the recent experiments made by Mr. Burke, will not find it very difficult to decide for themselves how far the results of these experiments justify all that has been said about them. They have only to ask themselves two or three simple questions, and to look for the answers in the facts of the case.

Does it appear that any one has prepared from sterilised bouillon by the action of radium, or in any other way, living organisms capable of multiplying either by repeated subdivision or by means of spores, or capable of producing definite fermentive changes such as those which we associate with so many of the organisms hitherto investigated? The answer jumps straight to the lips. No such discovery has been recorded ; nor has anything been observed which would justify us in supposing that we are on the verge of making such a discovery at the present moment. The fact is, that though much has been written, and among other things quite a big book, very little has really been accomplished up to the present. A few preliminary experiments suggested by the marvellous qualities of radium have been made, and that is all. These experiments and their results, which are not at all revolutionary, may be described in half-a-dozen sentences. Mr. Burke finds that when small quantities of radium bromide (or chloride) are scattered on the surface of carefully sterilised bouillon, well protected from the air in closed vessels, minute objects appear in the bouillon after one or two days. These objects have been watched, and Mr. Burke reports that after their first appearance they develop into two dots ; next, present the appearance of dumb-bells, and subsequently of biscuits ; afterwards take on forms which remind an observer of frog's spawn, and, finally, divide, lose their individuality and become transformed into minute crystals. These bodies, which Mr. Burke very prematurely describes as "cultures," do *not* multiply, as living organisms should do, when they are transferred to fresh tubes of sterilised bouillon, though, as might be expected, they give some slight evidence that the activity of the radium salt is not quite exhausted by its first action *and they are soluble in water.* Now it would be dogmatic to say that radium will not generate life in organic matter, but, clearly, Mr. Burke gives us little or no reason to suspect that it does so, at present. It would be unreasonable to assert that the experiments made by Mr. Burke were not worth trying, though, to most of us, they must appear not a little "mad," in the sense in which Sir William Ramsay used that word in a recent letter to *Nature*, and the observer of these phenomena does not himself, in his book, claim that he has produced life *de novo* in his test tubes, though he labels the objects he has studied with the misleading name of radiobes, and contends that by their discovery he has done something to bridge the gap between the living and non-living forms of matter. It is probable that very few would admit that even this latter contention can be seriously considered on the evidence before us, and thus there is no reason to excite ourselves overmuch about Mr. Burke's observations at present. A "mad" experiment is all very well, but there must be method in our madness, and in interpreting the results of "mad" experiments, and of all experiments, we must never forget the story of the Emperor's New Clothes. Mr. Burke's results teach no clear lesson. They convey no new knowledge about the origin of life. They leave us to-day where we stood a couple of years ago, and we must by no means shut our eyes to this fact.

THE END

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